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Europe's Initial Population Explosion

WILLIAM L. LANGER*

THE use of the dramatic term "explosion" in discussions of the present-day population problem may serve to attract attention and underline the gravity of the situation, but it is obviously a misnomer. The growth of population is never actually explosive, and as for the current spectacular increase, it is really only the latest phase of a development that goes back to the mid-eighteenth century.

Prior to that time the history of European population had been one of slow and fitful growth. It now took a sudden spurt and thenceforth continued to increase at a high rate. From an estimated 140,000,000 in 1750 it rose to 188,000,000 in 1800, to 266,000,000 in 1850, and eventually to 400,000,000 in 1900. The rate of increase was not uniform for all parts of the Continent, but

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it was everywhere strikingly high. Even in Spain, where there had been a remarkable loss of population in the seventeenth century, the population grew from 6,100,000 in 1725 to 10,400,000 in 1787 and 12,300,000 in 1833.[1]

This tremendous change in terms of European society has received far less attention from historians than it deserves. In the early nineteenth century it troubled the Reverend Thomas Malthus and precipitated a formidable controversy over the problem of overpopulation and the possible remedies therefor. But the discussion remained inconclusive until reopened in more recent times by British scholars, making use of the rather voluminous English records and directing their attention almost exclusively to their own national history. It is not unlikely that this focusing on the British scene has had the effect of distorting the issue, which after all was a general European one.

The point of departure for recent attacks on the problem was the publication, in the same year, of two closely related books: G. T. Griffith's *Population Problems in the Age of Malthus* (Cambridge, Eng., 1926) and M. C. Buer's *Health, Wealth and Population in the Early Days of the Industrial Revolution* (London, 1926). To these should be added the keen corrective criticism of T. H. Marshall's essay, "The Population Problem during the Industrial Revolution."[2]

Taken together, these writings provided a coherent, comprehensive analysis. Based on the proposition that the unusual increase of the population in the late eighteenth century was due primarily to a marked decline in the death rate, they attempted to show that this decline must, in turn, have been due to an alleviation of the horrors of war, to a reduction in the number and severity of famines, to an improvement in the food supply, and finally to a falling off of disease as a result of advancing medical knowledge and better sanitation.

These conclusions were not seriously challenged until after the Second World War, when a number of demographic and sociological analyses by British and American scholars called various items of the accepted theory seriously into question. Because of the inadequacy of the statistical data some aspects of the problem can probably never be disposed of definitively. However, the very foundation of the Griffith thesis has now been badly sapped. A number of specialists have come to the conclusion that the spectacular rise in the European population may have been due not so much to a reduction

[1] Albert Girard, "Le chiffre de la population de l'Espagne dans les temps modernes," *Revue d'histoire moderne*, IV (Jan.–Feb. 1929), 3–17. The growth of population was equally or even more spectacular in the United States and French Canada, to say nothing of China, but this paper considers only the problem as it emerged in Europe.

[2] T. H. Marshall, "The Population Problem during the Industrial Revolution," *Economic History*, I (Jan. 1929), 429–56.

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in the death rate as to a significant rise in the birth rate which, according to Griffith, did not vary greatly throughout the period.[3]

From these excellent studies of fertility and mortality there has not, however, emerged any satisfactory explanation to replace the argumentation of Griffith and Buer about underlying causes. It may not be amiss, then, for a historian to join the debate, even though he must disclaim at the outset any professional competence in demography or statistics.

From the strictly historical standpoint none of the previous interpretations of the initial spurt of the European population has been satisfactory. At the time it was commonly thought that the so-called "Industrial Revolution," with its high requirement for child labor, may have induced larger families.[4] This explication could at best apply primarily to Britain, where the demographic revolution was roughly contemporaneous with industrialization. Since the rate of population increase was just as striking in completely unindustrialized countries like Russia, a less parochial explanation was clearly required. At the present time it seems more likely that industrialization saved Europe from some of the more alarming consequences of overpopulation.[5]

Griffith's theses, inspired by Malthusian doctrine, are unacceptable, for the historical evidence provides little support for the notion of a marked decline in the death rate. Take, for instance, the mortality occasioned by war. Granted that no conflict of the eighteenth or early nineteenth centuries was as deadly as the Thirty Years' War is reputed to have been, there is yet no evidence of a difference so marked as to have made a profound change in the pattern of population. It is well known that nations usually recover quickly from the manpower losses of war. If it were not so, the bloody conflicts of the French revolutionary and Napoleonic periods should have had a distinctly retarding effect on the growth of the European population.

Not much more can be said of the argument on food supply. What reason is there to suppose that Europe suffered less from famine? We know that there were severe famines in the first half of the eighteenth century and that the years 1769–1774 were positively calamitous in terms of crop failures. The

[3] Halvor Gille, "The Demographic History of the Northern Countries," *Population Studies,* III (June 1949), 3–66; K. H. Connell, "Some Unsettled Problems in English and Irish Population History," *Irish Historical Studies,* VII (Sept. 1951), 225–34; H. J. Habakkuk, "The English Population in the Eighteenth Century," *Economic History Review,* 2d Ser., VI (Dec. 1953), 117–33; J. T. Krause, "Changes in English Fertility and Mortality, 1781–1850," *ibid.,* XI (Aug. 1958), 52–70; *id.,* "Some Implications of Recent Work in Historical Demography," *Comparative Studies in Society and History,* I (Jan. 1959), 164–88; Phyllis Deane and W. A. Cole, *British Economic Growth, 1688–1959* (Cambridge, Eng., 1962), 129–33.

[4] Joseph J. Spengler, "Malthus's Total Population Theory," *Canadian Journal of Economics and Political Science,* XI (Feb. 1945), 83–110.

[5] This question is well discussed in H. J. Habakkuk, "The Economic History of Modern Europe," *Journal of Economic History,* XVIII (Sept. 1958), 486–501.

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early 1790's and the years immediately following the peace in 1815 were almost as bad, while at much later periods (1837–1839, 1846–1849) all Europe suffered from acute food shortages. Even in Western and Central Europe famine was a constant threat until the railroads provided rapid, large-scale transportation.

Griffith was convinced that the important advances in agronomy (rotation of crops, winter feeding of cattle, systematic manuring, improved breeding of livestock, and so forth) as well as the practice of enclosure all made for more productive farming and greatly enhanced the food supply. But even in Britain, where agriculture was more advanced than elsewhere, these improvements did not make themselves generally felt until the mid-nineteenth century. There were many progressive landlords, on the Continent as in Britain, and no doubt there was improvement in grain production, but it was too slow, and grain imports were too slight to have had a decisive bearing on the rate of population growth. Even in mid-nineteenth-century Britain the three-field system was still prevalent, ploughs and other implements were old-fashioned and inefficient, grain was still cut by sickle or scythe and threshed with the flail, and ground drainage was primitive. Of course, more land had been brought under cultivation, but the available data reflect only a modest increase in the yield of grain per acre in this period.[6]

Crucial to the argumentation of Griffith and Buer was the proposition that improved health entailed a significant reduction in the death rate. The disappearance of bubonic plague, the falling off of other diseases, the advances in medical knowledge and practice (especially in midwifery), and progress in sanitation were in turn alleged to have produced the greater health of the people.

No one would deny that the disappearance of plague in the late seventeenth and early eighteenth centuries rid the Europeans of their most mortal enemy, and so reacted favorably on the development of the population. For the repeated plague epidemics had been fearfully destructive of life, especially in the towns. In the Black Death of 1348–1349 fully a quarter of the population had been carried away, while even as late as the epidemic of 1709–1710 from one-third to one-half of the inhabitants of cities such as Copenhagen and Danzig fell victims. In Marseilles in 1720 there were 40,000 dead in a total

[6] See esp. James Caird, *English Agriculture in 1850–1851* (2d ed., London, 1852), 474 ff.; R. E. P. Ernle, *English Farming, Past and Present* (6th ed., Chicago, 1961), 108, 135, 265, 357 ff.; G. E. and K. R. Fussell, *The English Countryman* (London, 1955), 126; H. W. Graf Finckenstein, *130 Jahre Strukturwandel und Krisen der intensiven europäischen Landwirtschaft* (Berlin, 1937); M. K. Bennett, "British Wheat Yield per Acre for Seven Centuries," *Economic History*, III (Feb. 1935), 12–29.

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population of 90,000. In Messina in 1743 over 60 per cent of the population was carried off.[7]

But whatever may have been the gains from the disappearance of plague they were largely wiped out by the high mortality of other diseases, notably smallpox, typhus, cholera, measles, scarlet fever, influenza, and tuberculosis. Of these great killers smallpox flourished particularly in the eighteenth century and tuberculosis in the eighteenth and nineteenth, while the deadly Asiatic cholera was a newcomer in 1830–1832.

Smallpox, though it reached up on occasions to strike adults, even of high estate, was primarily a disease of infancy and early childhood, responsible for one-third to one-half of all deaths of children under five. In 1721 the practice of inoculating children with the disease, in order to produce a mild case and create immunity, was introduced into England. It was rather widely used by the upper classes, but quite obviously had little effect on the epidemiology of the disease.[8] There appears to have been a gradual falling off of the disease after 1780, but even the introduction of vaccination by Edward Jenner in 1798 did not entirely exorcise the smallpox threat, though vaccination was offered gratuitously to thousands of children and was made compulsory in England in 1853. Mortality remained high, especially in the epidemics of 1817–1819, 1825–1827, 1837–1840, and 1847–1849. In the last great epidemic (1871–1872), when most people had already been vaccinated, the toll was exceedingly heavy: 23,062 deaths in England and Wales, 56,826 in Prussia in 1871 and 61,109 in 1872. Small wonder that opponents of vaccination stamped it a dangerous and futile procedure.[9]

Typhus, often associated with smallpox, attacked adults and was just as lethal. Like smallpox, it began to disappear only after 1870, to be replaced in part by measles, scarlet fever, and influenza.[10]

Tuberculosis, which no doubt was as old as human history, was the chief cause of premature deaths in the nineteenth century. It seems to have been widespread even in the mid-eighteenth century and continued so for well

[7] Karl F. Helleiner, "The Vital Revolution Reconsidered," *Canadian Journal of Economics and Political Science,* XXIII (Feb. 1957), 1–9, and more generally Hans Zinsser, *Rats, Lice and History* (New York, 1935); L. F. Hirst, *The Conquest of Plague* (London, 1953).

[8] Genevieve Miller, *The Adoption of Inoculation for Smallpox in England and France* (Philadelphia, 1957). For excellent general historical studies of smallpox, see Charles Creighton, *A History of Epidemics in Britain* (2 vols., Cambridge, Eng., 1891–94), II, Chap. IV; Alfons Fischer, *Geschichte des deutschen Gesundheitswesens* (2 vols., Berlin, 1933), II, 563 ff.; Jean Bourgeois-Pichat, "Évolution de la population française depuis le XVIII[e] siècle," *Population,* VI (Oct.–Dec. 1951), 635–62.

[9] W. Scott Tebb, *A Century of Vaccination* (2d ed., London, 1899), 58–59; David Johnston, *A History of the Present Condition of Public Charity in France* (Edinburgh, 1829), 539 ff.; and for the rest, Creighton, *Epidemics in Britain,* II, 606, and Fischer, *Deutsches Gesundheitswesen,* II, 556.

[10] See Creighton, *Epidemics in Britain,* for a detailed history of each of these diseases.

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over a hundred years.[11] But it was less spectacular than the terrifying cholera, which carried off half its victims within one to three days, and which struck Europe in four great epidemics during the nineteenth century. Paris in 1832 had 7,000 dead in eighteen days. Palermo in 1836–1837 lost 24,000 out of a population of 173,000. The epidemics of 1849 and 1866 were particularly lethal, especially on the Continent. Paris in three months of 1849 had 33,274 cases, of which 15,677 were fatal. Prussia in 1849 had 45,315 deaths, and in 1866, 114,683, while Russia in 1848–1849 registered over 1,000,000 dead.[12]

Considering the terrible and continuing ravages of disease in the days before the fundamental discoveries of Louis Pasteur and Robert Koch, it is hard to see how anyone could suppose that there was an amelioration of health conditions in the eighteenth century sufficient to account for a marked decline in the death rate.

Recent studies have pretty well disposed also of the favorite Griffith-Buer theme, that advances in medical knowledge and practice served to reduce mortality, especially among young children. Doctors and hospitals were quite incompetent to deal with infectious disease. The supposed reduction in child mortality was certainly not reflected in the fact that as late as 1840 half or almost half of the children born in cities like Manchester or even Paris were still dying under the age of five.[13]

Malthus thought the cities of his day better paved and drained than before, and this observation of the matter was exploited to the full by Griffith and Buer. Actually the improvements were mostly in the better sections of the towns, and Buer felt obliged to admit that living conditions were horrible, despite some amelioration. If one reviews these conditions even in the mid-nineteenth century, in any large European city—the dank cellar dwellings, the overcrowded courts, the vermin-infested rookeries, the filthy

[11] René and Jean Dubos, *The White Plague* (Boston, 1952), 6 ff.; Fischer, *Deutsches Gesundheitswesen,* II, 570 ff.; Arturo Castiglione, *History of Tuberculosis* (New York, 1933); S. R. Gloyne, *Social Aspects of Tuberculosis* (London, 1944); S. L. Cummins, *Tuberculosis in History* (Baltimore, 1949).

[12] The first great epidemic (1831–1832) has of late attracted a great deal of attention. See Sergei Gessen, *Cholernye Bunty, 1830–1832* (Moscow, 1932); *Le choléra: La première épidémie du* XIX^e^ *siècle,* ed. Louis Chevalier (Paris, 1958); R. E. McGrew, "The First Cholera Epidemic and Social History," *Bulletin of the History of Medicine,* XXXIV (Jan.–Feb. 1960), 61–73; Asa Briggs, "Cholera and Society in the Nineteenth Century," *Past and Present* (July 1961), 76–96. For the rest, see Francesco Maggiore-Perni, *Palermo e le sue grandi epidemie* (Palermo, 1894), 190, 244; Creighton, *Epidemics in Britain,* II, Chap. IX; Fischer, *Deutsches Gesundheitswesen,* II, 557; Georg Sticker, *Abhandlungen aus der Seuchengeschichte und Seuchenlehre* (2 vols., Giessen, 1908–12), II, 110 ff., 158 ff.; C. Macnamara, *A History of Asiatic Cholera* (London, 1876), 86 ff.

[13] Fischer, *Deutsches Gesundheitswesen,* II, 341, 369, 388 ff., and the fundamental articles of Habakkuk, "English Population in the Eighteenth Century," 117–33; Thomas McKeown and R. G. Brown, "Medical Evidence Related to English Population Changes in the Eighteenth Century," *Population Studies,* IX (Nov. 1955), 119–41; Richard H. Shryock, "Medicine and Society in the Nineteenth Century," *Journal of World History,* V (No. 1, 1959), 116–46.

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streets, the foul water supply—one can only shudder at the thought of what they may formerly have been. One can hardly persuade oneself that the improvements were such as to have effected a drop in the death rate.[14]

For Malthus "the whole train of common diseases and epidemics, wars, plague and famine" were all closely linked to "misery and vice" as positive checks to population growth. But misery and vice also included "extreme poverty, bad nursing of children, excesses of all kinds."

In this context it may be said that in Europe conditions of life among both the rural and urban lower classes—that is, of the vast majority of the population—can rarely have been as bad as they were in the early nineteenth century. Overworked, atrociously housed, undernourished, disease-ridden, the masses lived in a misery that defies the modern imagination. This situation in itself should have drastically influenced the population pattern, but two items in particular must have had a really significant bearing. First, drunkenness: this period must surely have been the golden age of inebriation, especially in the northern countries. The per capita consumption of spirits, on the increase since the sixteenth century, reached unprecedented figures. In Sweden, perhaps the worst-afflicted country, it was estimated at ten gallons of *branvin* and *akvavit* per annum. Everywhere ginshops abounded. London alone counted 447 taverns and 8,659 ginshops in 1836, some of which at least were visited by as many as 5,000–6,000 men, women, and children in a single day.[15]

So grave was the problem of intemperance in 1830 that European rulers welcomed emissaries of the American temperance movement and gave full support to their efforts to organize the fight against the liquor menace. To what extent drunkenness may have affected the life expectancy of its addicts, we can only conjecture. At the very least the excessive use of strong liquor is known to enhance susceptibility to respiratory infections and is often the determining factor in cirrhosis of the liver.[16]

Of even greater and more obvious bearing was what Malthus euphemisti-

[14] Diseases such as typhus and cholera were dirt diseases, carried often through contaminated water supply. Vienna secured an adequate water supply only in 1840; Hamburg in 1848; Berlin in 1852. In London there were still 250,000 cesspools in 1850; in Berlin only 9 per cent of all dwellings had water closets. For contemporary accounts, see Thomas Beames, *The Rookeries of London* (London, 1851); George Godwin, *London Shadows* (London, 1854); Fischer, *Deutsches Gesundheitswesen,* II, 500 ff.; Laurence Wright, *Clean and Decent: The Unruffled History of the Bathroom and the W.C.* (New York, 1960).

[15] James S. Buckingham, *History and Progress of the Temperance Reformation* (London, 1854), 28 ff.; Adolf Baer, *Der Alcoholismus* (Berlin, 1878), 196, 203 ff.

[16] On the liquor problem, see P. S. White and H. R. Pleasants, *The War of Four Thousand Years* (Philadelphia, 1846), 240 ff.; P. T. Winskill, *The Temperance Movement and Its Workers* (4 vols., London, 1891–92), I, Chap. IV; John C. Woolley and William E. Johnson, *Temperance Progress of the Century* (Philadelphia, 1905), Chap. XV; Johann Bergmann, *Geschichte der anti-Alkoholbestrebungen* (Hamburg, 1907), Chap. XII.

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cally called "bad nursing of children" and what in honesty must be termed disguised infanticide. It was certainly prevalent in the late eighteenth and nineteenth centuries and seems to have been constantly on the increase.[17]

In the cities it was common practice to confide babies to old women nurses or caretakers. The least offense of these "Angelmakers," as they were called in Berlin, was to give the children gin to keep them quiet. For the rest we have the following testimony from Benjamin Disraeli's novel *Sybil* (1845), for which he drew on a large fund of sociological data: "Laudanum and treacle, administered in the shape of some popular elixir, affords these innocents a brief taste of the sweets of existence and, keeping them quiet, prepares them for the silence of their impending grave." "Infanticide," he adds, "is practised as extensively and as legally in England as it is on the banks of the Ganges; a circumstance which apparently has not yet engaged the attention of the Society for the Propagation of the Gospel in Foreign Parts."

It was also customary in these years to send babies into the country to be nursed by peasant women. The well-to-do made their own arrangements, while the lower classes turned their offspring over to charitable nursing bureaus or left them at the foundling hospitals or orphanages that existed in all large cities. Of the operation of these foundling hospitals a good deal is known, and from this knowledge it is possible to infer the fate of thousands of babies that were sent to the provinces for care.[18]

The middle and late eighteenth century was marked by a startling rise in the rate of illegitimacy, the reasons for which have little bearing on the present argument. But so many of the unwanted babies were being abandoned, smothered, or otherwise disposed of that Napoleon in 1811 decreed that the foundling hospitals should be provided with a turntable device, so that babies could be left at these institutions without the parent being recognized or subjected to embarrassing questions. This convenient arrangement was imitated in many countries and was taken full advantage of by the mothers in question. In many cities the authorities complained that unmarried mothers from far and wide were coming to town to deposit their unwanted babies in the accommodating foundling hospitals. The statistics show that of the thousands of children thus abandoned, more than half were the offspring of married couples.

[17] Alexander von Öttingen, *Die Moralstatistik* (3d ed., Erlangen, 1882), 236 ff.

[18] In the years 1804–1814 the average annual number of births in Paris was about 19,500. Of these newcomers, roughly 4,700 were sent to the country by the *Bureau des Nourrices*, and another 4,000 were sent by the foundling hospital (*Maison de la Couche*). With the addition of children privately sent, it appears that a total of about 13,500 babies were involved. (Louis Benoiston de Chateauneuf, *Recherches sur les consommations . . . de la Ville de Paris* [Paris, 1821], 37.)

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There is good reason to suppose that those in charge of these institutions did the best they could with what soon became an unmanageable problem. Very few of the children could be cared for in the hospitals themselves. The great majority was sent to peasant nurses in the provinces. In any case, most of these children died within a short time, either of malnutrition or neglect or from the long, rough journey to the country.

The figures for this traffic, available for many cities, are truly shocking. In all of France fully 127,507 children were abandoned in the year 1833. Anywhere from 20 to 30 per cent of all children born were left to their fate. The figures for Paris suggest that in the years 1817–1820 the "foundlings" comprised fully 36 per cent of all births. In some of the Italian hospitals the mortality (under one year of age) ran to 80 or 90 per cent. In Paris the *Maison de la Couche* reported that of 4,779 babies admitted in 1818, 2,370 died in the first three months and another 956 within the first year.[19]

The operation of this system was well known at the time, though largely forgotten in the days of birth control. Many contemporaries denounced it as legalized infanticide, and one at least suggested that the foundling hospitals post a sign reading "Children killed at Government expense." Malthus himself, after visiting the hospitals at St. Petersburg and Moscow, lavishly endowed by the imperial family and the aristocracy, could not refrain from speaking out:

> Considering the extraordinary mortality which occurs in these institutions, and the habits of licentiousness which they have an evident tendency to create, it may perhaps be truly said that, if a person wished to check population, and were not solicitous about the means, he could not propose a more effective measure than the establishment of a sufficient number of foundling hospitals, unlimited as to their reception of children.

In the light of the available data one is almost forced to admit that the proposal, seriously advanced at the time, that unwanted babies be painlessly asphyxiated in small gas chambers, was definitely humanitarian.[20] Certainly the entire problem of infanticide in the days before widespread practice of contraception

[19] Léon Lallemand, *Histoire des enfants abandonnés et delaissés* (Paris, 1885), 207, 276. Among contemporary commentators, see Johnston, *Public Charity in France*, 319 ff.; Frederic von Raumer, *Italy and the Italians* (2 vols., London, 1840), I, 180 ff., 266; II, 80, 284; Richard Ford, *Gleanings from Spain* (London, 1846), Chap. XVII; and among later studies F. S. Hügel, *Die Findelhäuser und das Findelwesen Europas* (Vienna, 1863), 137 ff.; Arthur Keller and C. J. Klumper, *Säuglingsfürsorge und Kinderschutz in den europäischen Staaten* (2 vols., Berlin, 1912), I, 441 ff.; Joseph J. Spengler, *France Faces Depopulation* (Durham, N. C., 1938), 45 ff.; Roger Mols, *Introduction à la démographie historique des villes d'Europe du* XIV^e^ *au* XVIII^e^ *siècle* (3 vols., Louvain, 1954–56), II, 303 ff.; Krause, "Recent Work in Historical Demography," 164–88; Hélène Bergues, *La prévention des naissances dans la famille* (Paris, 1960), 17 ff.

[20] "Marcus" (pseudo.), *Essay on Populousness and on the Possibility of Limiting Populousness* (London, 1838). The quotation from Malthus is in the last edition of his *Essay* (6th ed., London, 1826), reprinted by G. T. Bettany (London and New York, 1890), 172.

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deserves further attention and study. It was undoubtedly a major factor in holding down the population, strangely enough in the very period when the tide of population was so rapidly rising.

Summing up, it would seem that in the days of the initial population explosion one can discern many forces working against a major increase and few if any operating in the opposite direction. It is obviously necessary, then, to discover one or more further factors to which a major influence can fairly be attributed.

If indeed the birth rate was rising, this was presumably due primarily to earlier marriage and to marriage on the part of a growing proportion of the adult population. Even slight variations would, in these matters, entail significant changes in the birth rate.[21]

Unfortunately the marriage practices of this period have not been much investigated. Under the feudal system the seigneur frequently withheld his consent to the marriage of able-bodied and intelligent young people whom he had selected for domestic service in the manor house. Likewise under the guild system the master had authority to prevent or defer the marriage of apprentices and artisans. Whether for these reasons or for others of which we have no knowledge, there appears to have been a distinct decline in the number of marriages and a rise in the age of marriage in the late seventeenth and early eighteenth centuries. Some writers have even spoken of a "crise de nuptialité" in this period. But by the mid-eighteenth century the old regime was breaking down, soon to be given the *coup de grâce* by the French Revolution. With the personal emancipation of the peasantry and the liquidation of the guild system, the common people were freer to marry, and evidently did so at an early age. There is, in fact, some indication that the duration of marriages was extended by as much as three years, at least in some localities.[22]

The rapid increase of the population was at the time often attributed to these changes, and before long a number of German states tried to counter the trend by laws specifically designed to restrict marriage: men were refused marriage licenses until they were thirty and received them then only if they could show that they had learned a trade and had a job waiting for them. Those who had been on relief in the preceding three years were denied a license on principle. Under these circumstances it is altogether likely that many of the young people who emigrated from Germany in these years did so chiefly in order to get married.[23]

[21] This aspect is rightly stressed by Habakkuk, "English Population in the Eighteenth Century," 117–33.

[22] On the problem of marriage, see esp. the excellent discussion in Mols, *Démographie historique*, II, 267 ff.

[23] A. S. (Alexander Schneer), "Über die Zunahme der Bevölkerung in dem mittleren Europa und die Besorgnisse vor einer Überbevölkerung," *Deutsche Vierteljahrschrift*, III (1844), 98–

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Marriage practices, though obviously important, seem hardly to provide a complete explanation of the population growth. To discover a further, possibly decisive factor, it is necessary to return to consideration of the food supply, recalling the proposition advanced by the physiocrats and heavily underlined by Malthus, that the number of inhabitants depends on the means of subsistence—more food brings more mouths.[24] That population tends to rise and absorb any new increment of the food supply is familiar to us from the history of underdeveloped societies. Historically it has been demonstrated by studies of the relationship between harvest conditions on the one hand and marriage and birth rates on the other. In Sweden, for example, where careful statistics were kept as long ago as the seventeenth century, the annual excess of births over deaths in the eighteenth century was only 2 per thousand after a poor crop, but 6.5 after an average harvest, and 8.4 after a bumper crop. Invariably, and as late as the mid-nineteenth century, high wheat prices have been reflected in a low marriage and to some extent in a low birth rate.[25]

The addition of an important new item to the existing crops would necessarily have the same effect as a bumper crop. Such a new item—one of the greatest importance—was the common potato, a vegetable of exceptionally high food value, providing a palatable and satisfying, albeit a monotonous diet. Ten pounds of potatoes a day would give a man 3,400 calories—more than modern nutritionists consider necessary—plus a substantial amount of non-animal protein and an abundant supply of vitamins.[26] Furthermore, the potato could be grown on even minute patches of poor or marginal land, with the most primitive implements and with a minimum of effort. Its yield was usually abundant. The produce of a single acre (the equivalent in food value of two to four acres sown to grain) would support a family of six or even

141; Wilhelm G. Roscher, *Die Grundlagen der Nationalökonomie* (Stuttgart, 1854), 490 ff. The eminent jurist, Robert von Mohl, considered antimarriage laws indispensable unless the poorer classes exercised prudence in marriage. See D. V. Glass, "Malthus and the Limitation of Population Growth," in his *Introduction to Malthus* (London, 1953), 25–54.

[24] Richard Cantillon, *Essai sur la nature du commerce en général* (London, 1755), argued that an increase in subsistence would positively provoke a rise in the population; Malthus wrote: "The only true criterion of a real and permanent increase in the population of any country is the increase of the means of subsistence." (*Essay*, 6th ed., 294.)

[25] E. E. Heckscher, "Swedish Population Trends before the Industrial Revolution," *Economic History Review*, 2d Ser., II (No. 3, 1950), 266–77; Dorothy S. Thomas, *Social and Economic Aspects of Swedish Population Movements, 1750–1933* (New York, 1941), 81 ff.; Jean Mevret, "Les crises de subsistance et la démographie d'ancien régime," *Population*, I (Oct.–Dec. 1946), 643–50; F. G. Dreyfus, "Prix et population à Trèves et à Mayence au XVIII[e] siècle," *Revue d'histoire économique et sociale*, XXXIV (No. 3, 1956), 241–61; C. H. Pouthas, *La population française pendant la première moitié du XIX[e] siècle* (Paris, 1956), 29; Louis Chevalier, *Démographie générale* (Paris, 1951), 338–39.

[26] Redcliffe N. Salaman, *The History and Social Influence of the Potato* (Cambridge, Eng., 1949), 122 ff.; K. H. Connell, *The Population of Ireland, 1750–1845* (Oxford, Eng., 1950), 151 ff.

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eight, as well as the traditional cow or pig, for a full year. The yield in terms of nutriment exceeded that of any other plant of the Temperate Zone.[27]

The qualities of the potato were such as to arouse enthusiastic admiration among agronomists and government officials. It was spoken of as "the greatest blessing that the soil produces," "the miracle of agriculture," and "the greatest gift of the New World to the Old." The eminent Polish poet, Adam Mickiewicz, writing as a young man in the hard and hungry years following the Napoleonic Wars, composed a poem entitled *Kartofla,* celebrating this humble vegetable which, while other plants died in drought and frost, lay hidden in the ground and eventually saved mankind from starvation.[28]

The history of the potato in Europe is most fully known as it touches Ireland, where in fact it became crucial in the diet of the people. It was introduced there about the year 1600 and before the end of the seventeenth century had been generally adopted by the peasantry. By the end of the eighteenth century the common man was eating little else:

> Day after day, three times a day, people ate salted, boiled potatoes, probably washing them down with milk, flavouring them, if they were fortunate, with an onion or a bit of lard, with boiled seaweed or a scrap of salted fish.[29]

Because this was so, Ireland provides a simple, laboratory case. There were in Ireland no industrial revolution and no war, but also no fundamental change in the pattern of famine or disease. The unspeakable poverty of the country should, it would seem, have militated against any considerable population increase. Yet the population did increase from 3,200,000 in 1754 to 8,175,000 in 1846, not counting some 1,750,000 who emigrated before the great potato famine of 1845–1847.[30]

It was perfectly obvious to contemporaries, as it is to modern scholars, that this Irish population could exist only because of the potato. Poverty-stricken though it might be, the Irish peasantry was noteworthy for its fine physique. Clearly people were doing very well physiologically on their potato fare. Young people rented an acre or less for a potato patch. On the strength of this they married young and had large families.

[27] On its qualities, see the detailed report of Antoine Parmentier, *Examen chymique des pommes de terre* (Paris, 1773), 3; also Berthold Laufer, "The American Plant Migration: Part II, the Potato," *Field Museum of Natural History,* Anthropological Ser., XXVIII (July 1938), 11.

[28] *Adam Mickiewicz, Poet of Poland,* ed. Manfred Kridl (New York, 1951), 242 ff.; see also Henry Phillips, *The History of Cultivated Vegetables* (2d ed., 2 vols., London, 1822), II, 85 ff.; Georges Gibault, *Histoire des légumes* (Paris, 1912), 243 ff.

[29] K. H. Connell, "The Potato in Ireland," *Past and Present* (Nov. 1962), 57–71. Salaman, *History and Social Influence of the Potato,* is little short of an economic-social history of the British Isles; on Ireland, see esp. Chaps. XI–XVI.

[30] Connell, *Population of Ireland, passim.*

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So impressive was the role of the potato in Ireland that Arthur Young, in *The Question of Scarcity Plainly Stated and Remedies Considered* (London, 1800), urged the British government, as a hedge against failure of the grain crop, to endow every country laborer who had three or more children with a half acre of land for potatoes and enough grass to feed one or two cows: "If each had his ample potato-ground and a cow, the price of wheat would be of little more consequence to them than it is to their brethren in Ireland."

Malthus at once objected to this proposed remedy for want. Young's system, he argued, would operate directly to encourage marriage and would be tantamount to a bounty on children. Potatoes tended to depress wages and living standards by making possible an increase in the population far beyond the opportunities of employment.[31]

Why should not the impact of the potato have been much the same in Britain and on the Continent as in Ireland? If it made possible the support of a family on a small parcel of indifferent soil, frequently on that part of the land that lay fallow, and thereby encouraged early marriage, why should it not in large part explain the unusual rise in the population anywhere?

A definitive answer is impossible partly because the history of potato culture has not been intensively studied, and partly because the situation in other countries was rarely if ever as simple or as parlous as that of Ireland.[32] The most nearly comparable situation was that obtaining in the Scottish Highlands and the Hebrides, where the potato proved to be "the most beneficial and the most popular innovation in Scottish agriculture of the eighteenth century." By 1740 the potato had become a field crop in some sections, grown in poor soil and sand drift and soon becoming the principal food of the population, much as in Ireland. In these areas also the spread of potato culture ran parallel to a marked expansion of the population.[33]

In the Scottish Lowlands, as in England, the potato met with greater resistance. Scottish peasants hesitated to make use of a plant not mentioned in the Bible, and it was feared in many places that the potato might bring on leprosy. In southern England in particular, the peasants suspected that the potato would tend to depress the standard of living to the level of that of the Irish. Nonetheless the potato, having in the early seventeenth century been a

[31] Thomas Malthus, *Essay on the Principle of Population* (London, 1798; 2d enlarged ed., 1803), Bk. I, Chap. II, 7.

[32] Salaman's lengthy and valuable study is by no means as comprehensive as the title would suggest. It is, in fact, restricted to a history of the potato in the British Isles.

[33] James E. Handley, *Scottish Farming in the Eighteenth Century* (London, 1953), Chap. VIII; see also Malcolm Gray, "The Highland Potato Famine of the 1840's," *Economic History Review*, 2d Ser., VII (Apr. 1955), 357–68.

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delicacy grown in the gardens of the rich, was strongly urged in the 1670's as a food for the poor. In Lancashire it was grown as a field crop before 1700. During the ensuing century it established itself, even in the south, as an important item in the peasant's and worker's diet. The lower classes continued to prefer wheat bread, but growing distress forced the acceptance of the potato which was, in fact, the only important addition to the common man's limited diet in the course of centuries.[34] Long before the end of the eighteenth century large quantities of potatoes were being grown around London and other large cities. By and large the spread of the potato culture everywhere corresponded with the rapid increase of the population.[35]

Much less is known of the potato's history on the Continent. It was introduced in Spain from South America in the late sixteenth century and quickly taken to Italy, Germany, and the Low Countries. As in England, it was cultivated by the rich in the seventeenth century and gradually adopted by the common people in the eighteenth. It appears to have been grown quite commonly in some sections of Saxony even before the eighteenth century, while in some parts of southern Germany it became common in the period after the War of the Spanish Succession. In several instances soldiers campaigning in foreign lands came to know and appreciate its qualities.

One of the greatest champions of the potato was Frederick the Great, who throughout his reign kept urging its value as food for the poor, prodding his officials to see that it was planted by the peasants, and providing excellent instructions as to its culture and preparation. He met at first with much resistance, but after the crop failures of 1770 and 1772 even the most hidebound peasantry came to accept it. They were impressed by the fact that the potato thrived in wet seasons, when the wheat crop suffered, and that the potato did well in sandy soil. They also realized that it would make an excellent salad and that it went exceptionally well with herring.

By the beginning of the nineteenth century the potato was already a major field crop in Germany, especially in Prussia, Posen, Pomerania, and Silesia. By the mid-century the per capita consumption in Prussia was nine bushels per annum, and potato production almost equaled in volume the production of all other cereals taken together.[36]

[34] G. E. Fussell, "The Change in Farm Labourers' Diet during Two Centuries," *Economic History*, I (May 1927), 268–74; Jack C. Drummond and Anne Wilbraham, *The Englishman's Food* (2d ed., London, 1958), 208 ff.

[35] Salaman, *History and Social Influence of the Potato*, Chaps. XXIII–XXVI and the interesting chart on p. 538; see also Philip Miller, *The Gardener's Dictionary* (6th ed., London, 1752); Ernest Roze, *Histoire de la pomme de terre* (Paris, 1898); Gibault, *Histoire des légumes*.

[36] See the detailed and appreciative account in Johann G. Krünitz, *Ökonomisch-technologische Encyklopedie* (181 pts., Berlin, 1778–1843), Pt. 35, 232–412; Curt Dietrich, *Die Entwicklung des Kartoffelfeldbaues in Sachsen* (Merseburg, 1919), 10 ff. The various instructions and orders

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The Austrian government followed Frederick's lead and succeeded in securing the adoption of the potato in the German parts of the monarchy. Galicia, Bohemia, and Hungary became major centers of potato production.[37] In France, too, potato culture had become established in the eastern provinces, such as Lorraine, Alsace, and Burgundy. In 1770 the eminent pharmacist and chemist, Antoine Parmentier, who had become acquainted with the vegetable in Germany during the Seven Years' War, won the prize offered by the Besançon Academy for an essay on the best vegetable to use as a substitute for wheat in times of food shortage. Parmentier was certainly not responsible for the introduction of the potato into France, but he proved himself an able promoter and succeeded in securing the support of Louis XVI. He tells us that in the early 1770's the markets of Paris were already full of potatoes and that they were sold raw or roasted on the streets, much like chestnuts.[38]

By 1800, then, the common people in the Netherlands as in the British Isles, Germany, and Scandinavia were eating potatoes twice a day, and even the French peasantry (passionately devoted to white wheat bread) was rapidly capitulating. In the early nineteenth century French potato production increased from 21,000,000 hectoliters in 1815 to 117,000,000 in 1840. This, be it noted, was a period when the French population was still increasing.[39]

A few words should, perhaps, be said about Eastern Europe. The rate of population growth in the Russian Empire appears to have been higher than in any other continental country. The population increased from about 16,000,000 in 1745 to 37,500,000 in 1801 to 62,000,000 in 1852. Part of this increase

of Frederick the Great are printed in Rudolph Stadelmann, *Preussens Könige in ihrer Thätigkeit für die Landescultur* (4 vols., Leipzig, 1878–87), II, Nos. 144, 158, 186, 258, 294; see further Theodor Freiherr von der Goltz, *Geschichte der deutschen Landwirtschaft* (2 vols., Berlin, 1902), I, 455 ff.; Hans Lichtenfelt, *Die Geschichte der Ernährung* (Berlin, 1913), 95; Kurt Hintze, *Geographie und Geschichte der Ernährung* (Leipzig, 1934), 98 ff.; Kurt Hanefeld, *Geschichte des deutschen Nährstandes* (Leipzig, 1935), 297 ff.; C. F. W. Dieterici, *Handbuch der Statistik des Preussischen Staates* (Berlin, 1861), 264 ff.; Hans W. Graf Finck von Finckenstein, *Die Entwicklung der Landwirtschaft in Preussen und Deutschland* (Göttingen, 1960).

[37] See the scholarly analysis of Ignaz Hübel, "Die Einführung der Kartoffelkultur in Niederösterreich," *Unsere Heimat,* New Ser., V (Mar. 1932), 69–78; Friedrich W. von Reden, *Deutschland und das übrige Europa* (Wiesbaden, 1854), 151.

[38] Parmentier, *Examen chymique des pommes de terre,* 5, 186. Already in 1755 Henri-Louis Duhamel du Monceau (*Traité des cultures des terres* [6 vols., Paris, 1750–51]) had urged the value of the potato in times of want, and Turgot as well as the philosophes had appealed to the people to abandon their superstitions and prejudicies. (See Gibault, *Histoire des légumes,* 243 ff.)

[39] B. H. Slicher van Bath, *De agrarische Geschiedenis van West-Europa, 500–1850* (Utrecht, 1960), 291 ff.; Paul Lindemans, *Geschiedenis van de Landbouw in België* (2 vols., Antwerp, 1952), II, 182 ff. On France, see Benoiston-Chateauneuf, *Recherches sur les consommations . . . de la Ville de Paris,* 99; Charles Dupin, *Les forces productives et commerciales de la France* (2 vols., Paris, 1827), II, 194, 208; Sébastien Charléty, *La Monarchie de Juillet* (Paris, 1921), 190; Henri Sée, *Histoire économique de la France* (2 vols., Paris, 1948–51), II, 181. As late as 1837, however, Stendahl (*Mémoires d'un touriste* [Paris, 1837; new ed., 2 vols., Paris, 1953]) noted that in some sections of France the peasants still looked down on those who subsisted chiefly on potatoes.

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was of course due to the substantial territorial acquisitions of Catherine the Great and Alexander I. Yet the territory of 1725 saw a rise from 14,000,000 in that year to 45,000,000 in 1858.[40] In this case the population growth seems indeed to have been due to an exceptionally high birth rate. The death rate too was high (about 39.4 per 1,000 in the period 1840–1860), but the birth rate was substantially higher (49.7 per 1,000 from 1841 to 1850, and 52.4 per 1,000 from 1851 to 1860).

Information available on the culture of the potato in Russia is not sufficient to warrant any firm conclusion. Russian armies became acquainted with the vegetable in Germany during the Seven Years' War, at which time it seems to have been already well established in Poland and the Baltic Provinces. During a famine and epidemic in 1765 a board of medical advisers convinced Catherine the Great and her government of the importance of the potato as a preventive of famine and typhus. The government thereupon embarked on a systematic campaign of propaganda with the result that by 1800 the potato was widely cultivated in the Ukraine and the western *gubernias*. In many areas, however, the superstitions of the peasantry proved almost insurmountable. It was only after the crop failures of 1838–1839, when Tsar Nicholas reinforced the earlier efforts to further its adoption, that it became a key crop in central Russia also. By 1900 Russia was second only to Germany as a potato-producing country.[41]

Any conclusion to be drawn from these data must be tentative. The great upswing in the European population beginning around the middle of the eighteenth century can never be explained with any high degree of assurance or finality. It is extremely difficult to demonstrate whether it was due primarily to a decline in the death rate or to a rise in the birth rate. And beyond any such demonstration would lie the further question of the forces making for such demographic change. It is most unlikely that any single factor would account for it. Thus far the many explanations that have been advanced seem woefully inadequate. It seems altogether probable, therefore, that the introduction and general adoption of the potato played a major role. Its establishment as a field crop and as a basic food item of the general population coincided roughly with the sudden spurt of the population. Furthermore, it would appear that the areas of the most intensive potato culture such as Ire-

[40] There are substantial discrepancies in the figures given by various authors. See Ludwik de Tegoborski, *Études sur les forces productives de la Russie* (3 vols., Paris, 1852–55), and the English translation, *Commentaries on the Productive Forces of Russia* (London, 1855); see also the discussion in Jerome Blum, *Lord and Peasant in Russia from the Ninth to the Nineteenth Century* (Princeton, N. J., 1961), 278.

[41] Tegoborski, *Études*, II, 104 ff.; Baron August von Haxthausen, *The Russian Empire* (2 vols., London, 1856), II, 410, 425; and the exhaustive study of V. C. Lekhnovich, "K Istorii Kulturi Kartofelia v Rossii," *Materiali po Istorii Zemledeliia CCCR*, II (1956), 248–400.

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land, the Scottish Highlands, Lancashire, and western and southwestern Germany were also the areas of exceptionally rapid population increase, population pressure, and early emigration.

So much at least seems clear: that marriage was easier in the generations before and after 1800 than in earlier times, and that there was a much better opportunity for men and women to marry at an early age. For the fact that on a pathetically small patch of ground one could grow in potatoes from two to four times as much food as one could in terms of wheat or other grains, enough indeed to feed a family of more than average size was, I submit, a major revolutionary innovation in European life. In 1844 the eminent German agronomist, Baron August von Haxthausen, noted that the introduction of the potato "has undoubtedly produced immense effects upon Europe, in the moulding and culture of which it has probably operated more powerfully than any other material object." A few years later the equally authoritative German economist, Wilhelm Georg Roscher, declared without qualification that the adoption of the potato had resulted in a rapid growth of population.[42]

Perhaps the time has come, then, for historians to pay greater attention to the evolution of the human diet and its social consequences. As a first step, more intensive research might be initiated to test whether so startling a new departure in European history as the initial population explosion is to be attributed at least in large part to so drastic a change in the people's food as the advent of the common potato.

[42] Haxthausen, *Russian Empire,* II, 425; Roscher, *Grundlagen der Nationalökonomie,* 438.

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The Revolutionary-Napoleonic Era: How Much of a Watershed?

FRANKLIN L. FORD *

EVERY student of history, regardless of his special field, faces a problem of choice, endlessly recurring but constantly recast. Put in terms that seem to us worn only because none of us can wholly avoid using them, the choice is between emphasis on continuity and emphasis on change. No matter how he may hedge or qualify, the historian, before he completes an investigation, must respond to the question Carl Becker used to ask his seminar at Cornell: "Should we finally insist that while the world may change, it changes slowly —or that while the world may change slowly, it does change?" Everything in our professional training supports the first proposition, yet our fascination with historical process would forsake us if we did not also believe the second.

Actually, of course, real sophistication implies an ability to keep both tendencies in view, to exploit within our own minds the dialectic between solid evidence of continuity and unblinkable signs of change. It is equally clear that one may tip the balance this way or that in his analysis of a particular situation without thereby assuming a rigid posture in defense either of change or of continuity as the key to all understanding of the past. Hence, I assume that I may express some views on the European record from 1789 to 1815 and yet not be classified hereafter, forever, and in all things as "either a little Liberal, or else a little Conservative."

A number of powerful arguments have been leveled against the view, early expressed, though for very different reasons, by both Leopold von Ranke and Alexis de Tocqueville, to the effect that in the years after 1789 an older Europe collapsed and that a new one, ominous in its strangeness, came into being. Various scholars have warned us that to interpret the revolutionary-Napoleonic era as a chasm over which no one could cross from the eighteenth century into the nineteenth, or as a wall of flame through which nothing could pass intact, would be to distort reality beyond recognition.

* This article was originally delivered as a paper before the Modern European History Section at the American Historical Association's annual meeting, the Conrad Hilton Hotel, Chicago, Illinois, December 29, 1962. The author is professor of history and dean of the Faculty of Arts and Sciences, Harvard University. Many of the ideas here set forth will receive fuller development in the author's forthcoming volume on the period 1780 to 1830 in "The Longmans General History of Europe."

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Against even the more modest image of a revolutionary "watershed" the arguments are impressive. They are also, in my view, an essential antidote to the kind of oversimplification to which such an image may lend itself. It should be noted, however, that not all of these arguments go to the question of continuity, strictly defined. Some of them attempt only to show that particular characteristics often assigned to the nineteenth century were anticipated in at least the later decades of the eighteenth. Conversely, some others refer to instances of reaction toward pre-1789 conditions in the years after 1815, when they do not center on the reign of Napoleon I, himself. In both clusters of interpretation, what is involved is not really a demonstration of statics, but rather a series of disputes over the timing and the direction of particular changes in the European scene. I do not suggest that one kind of emphasis, whether it be on long-term continuity, on eighteenth-century anticipation of the nineteenth century, or on nineteenth-century reversions toward the past, is necessarily the most significant. I only suggest that they *are* different kinds of emphasis. All three have tended, nevertheless, to merge into a formidable case against any theory that stresses the cleavage between the political forms, the diplomatic patterns, the social structure, the economic conditions, the influential ideas of Europe before 1789, on the one hand, and those of Europe after 1815 on the other.

As most commonly formulated, this case rests on the assertion that it was the seventeenth century, sometimes extended to include the first years of the eighteenth, which saw the fundamental break between an old and a "modern" Europe. Once this great change in both ideas and institutions had occurred, so runs the argument, there ensued a period extending into the early twentieth century, a period best envisaged as a plateau, marked by hummocks and creek beds, but never divided by a major watershed. Specifically, the revolutionary-Napoleonic interlude appears to shrink to the dimensions of an overrated episode the dust from which, once it settled, proved to have been just that—dust, having no deep significance for the substratum of continuity in human affairs.

The image of a "modern plateau" is nowhere more gracefully suggested than in Paul Hazard's *Crise de la conscience européenne* (Paris, 1935).[1] This brilliant synthesis of the intellectual ferment between 1680 and 1715 incorporates several different levels of interpretation, the most obvious of which has to do with the period's own singular importance in the odyssey of European thought. More immediately relevant here, however, is Hazard's implicit con-

[1] The English translation by J. R. May of Hazard's book bears the title *The European Mind: The Critical Years, 1680–1715* (New Haven, Conn., 1953).

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viction that with Locke, Newton, and Bayle, to name only three protagonists, Western civilization embarked upon two centuries of discussion, acrimonious and often divergent to be sure, but confined within a shared system of rational assumptions, mechanical models, and empirical-analytic methods. Not until the antirationalistic revolt of the early twentieth century would this system be attacked at its foundations.

Now, given this general conception, what special importance in the history of ideas can be assigned the noisy crisis midway through those two hundred or more years? What point is there in denying that both Marx and Darwin were in their time just as recognizable philosophes as Voltaire and Gotthold Lessing had been in theirs? Frank Manuel, in *The Prophets of Paris,* has recently spelled out the striking similarities, if not in character or rhetoric, at least in social concern and a certain urgency, at once chiliastic and self-important, that connect a series of eighteenth- and nineteenth-century French thinkers, from Turgot through Condorcet, Saint-Simon, and Fourier to Comte.[2] Neither Hazard nor Manuel would deny, of course, that the revolutionary crisis constituted a major chapter in the history of modern intellectual traditions, but both, if I understand them, would stress the solidity of those traditions in the face of numberless shocks and challenges. To many other historians of ideas, it has seemed increasingly difficult to take very seriously any departure from the basic premises bequeathed by the giants Hazard treats until, at the turn of our own century, Einstein repudiated Newton, while Locke's ideas were assaulted on the political front by Vilfredo Pareto and Nikolai Lenin, allies in nothing else, and on the psychological, by Sigmund Freud.

If the argument ended here, we might conclude that what confronts us is only an interesting example of continuity at the level of ideas, cutting across the political, social, and economic changes of the late eighteenth and early nineteenth centuries. But the case against the watershed metaphor, as applied to that period, is much broader, too broad in fact to be assigned to the history of abstract thought and thus fitted into a neat jurisdictional compromise. Perhaps the earliest argument for continuity was published by Albert Sorel almost eighty years ago in the first volume of his *L'Europe et la Révolution française,* which stressed the tenacious ambitions governing the intercourse of sovereign states from the eighteenth century through the revolutionary age.[3] Readers of John U. Nef's *War and Human Progress*[4] will recall his rejection of the older, more cataclysmic vision of an "Industrial Revolution."

[2] Frank Manuel, *The Prophets of Paris* (Cambridge, Mass., 1962).
[3] Albert Sorel, *L'Europe et la Révolution française* (8 vols., Paris, 1885–1904).
[4] John U. Nef, *War and Human Progress* (Cambridge, Mass., 1950), 273–76.

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According to Nef, the crucial economic changes by which Europe and the rest of the world have indeed been revolutionized cannot be assigned to the years 1760–1832, mentioned in the titles or prefaces of countless books on the subject. Rather, those changes may be seen in the offing by the mid-seventeenth century, accelerating unevenly both in England and in France during the eighteenth, reaching literally earth-shaking intensity in Germany and the United States by the mid-nineteenth. We should add Russia and Japan in the first half of the twentieth century, and presumably China and India in its second half.

It seems to me to have been demonstrated beyond question that the development both of ideas and of institutions from what most of us still call early modern into modern times must today be discussed, if not in terms of flat continuity, at least in terms of complex, irregular, and in some areas exceedingly slow change. The influence of this demonstration on historical thinking has been reinforced, as earlier noted, by examples of both the eighteenth century's foreshadowing the nineteenth, and the latter's turning backward toward the former. We have been warned, for instance, that the preconditions for an outburst of popular nationalism existed long before that force was unleashed by revolutionary France.[5] We have been reminded that Rousseau, Diderot, and the young Germans of the *Sturm und Drang* in the 1770's adumbrated many of the characteristics of later romanticism.[6] Looking in the opposite direction, we have had to examine the revival of ecclesiastical power after the long humiliation of the papacy, a revival symbolized in 1814 by the rehabilitation of the Society of Jesus. In a different quarter, we have been shown how many of the administrative and social changes launched by the Prussian reformers of Baron vom Stein's era had been undone by 1819 at the latest.[7] Finally, we reflect that the dynastic principle, challenged by mobs and by political theorists, perverted by a Corsican family's willingness to serve, was in 1815 firmly reasserted in France, Spain, and the Italian principalities, extended to the Netherlands, and given new scope in the kingdoms and grand duchies of southern Germany.

Taken by themselves, all these affirmations and reminders seem to leave us no choice but to abandon the conception of a revolutionary-Napoleonic "watershed," advising Tocqueville and Ranke that they were standing too close to what they thought was the beginning of a new age—and that hence

[5] See, e.g., Boyd C. Shafer, *Nationalism: Myth and Reality* (New York, 1955), Chaps. VII and VIII; and Koppel S. Pinson, *Pietism as a Factor in German Nationalism* (New York, 1934).

[6] Paul Hazard, *La pensée européenne au* XVIII^ème^ *siècle* (3 vols., Paris, 1946), esp. Pt. 2, Bk. II (Eng. trans. by J. R. May, *The European Mind: The Eighteenth Century* [New Haven, Conn., 1954]); H. A. Korff, *Geist der Goethezeit* (5 vols., Leipzig, 1923–57).

[7] W. M. Simon, *The Failure of the Prussian Reform Movement* (Ithaca, N. Y., 1955).

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they quite naturally exaggerated its significance. It is just at this point, however, that I must define a view which will be developed in the remainder of these remarks, for I believe that it is not enough simply to recognize important strands of continuity linking the eighteenth century to the period that followed. When all due attention has been paid to those strands—and surely the continuity of human life, the overlapping of generations, was the most important of all—it still is not clear that we must slide into suggesting to ourselves, our readers, or our students that the revolutionary-Napoleonic era really did not make much difference. On the contrary, it is time again to emphasize certain changes witnessed by that era, changes so fundamental that no historian can move from eighteenth- to nineteenth-century subject matter without major adjustments in his conception of Europe and in the terms he must use. It is time to say, if I may in a sense use Alfred Cobban's phrase against him, that the Revolution was no "myth." [8]

Enough authorities, be it said at once, have lent support to this view so that no one, in adopting it, should pose as a lonely dissenter. My own thinking has benefited greatly from recent works by R. R. Palmer and Richard Herr.[9] The British author of a new volume on the eighteenth century, M. S. Anderson, concludes his work: "By the 1780's modern history in the genuine as opposed to the textbook sense of the term was beginning." [10] All I can hope to add to these and other studies is my own way of dissecting the process of change and of estimating its importance in the period under discussion.

There are several quite different types of evidence to which we might usefully address ourselves. Who can deny, for example, that nearly every major European government came out of the revolutionary-Napoleonic crisis with its administrative organization profoundly and, as time was to show, irreversibly altered? After 1815 France retained a set of budgetary procedures, a network of departmental prefects, and a system of centrally appointed judges in place of the deficit financing ex post facto, the quaint chaos of provincial

[8] In fairness to Alfred Cobban, whose inaugural lecture as Professor of French History in the University of London was entitled *The Myth of the French Revolution* (London, 1955), it should be said that one of several "myths" he attacks in that paper is the simplistic notion that there was *a* French Revolution. Instead, as he says quite rightly, the historian must deal with a whole generation of turmoil involving disparate, or in some instances conflicting, elements and directions. Cobban's much-discussed essay, nevertheless, by emphasizing the extent to which the Revolution was "the child of the eighteenth century" (p. 21) and by casting doubt on the reality of fundamental, especially institutional changes resulting from it, appears to support the general line of interpretation I am here attempting to counterbalance.

[9] R. R. Palmer, *The Age of the Democratic Revolution: A Political History of Europe and America, 1760–1800. The Challenge* (Princeton, N. J., 1959); Richard Herr, *The Eighteenth Century Revolution in Spain* (Princeton, N. J., 1958).

[10] M. S. Anderson, *Europe in the Eighteenth Century, 1713–1783* (London, 1961), 338. An exceedingly valuable extension of this argument to our own history appears in the article by Bernard Bailyn, "Political Experience and Enlightenment Ideas in Eighteenth-Century America," *American Historical Review*, LXVII (Jan. 1962), 339–51.

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powers, and the court system based on ownership of office with which the old monarchy had lived for centuries. But it was not only the brilliant and terrible homeland of the Revolution that changed its methods of conducting governmental business. The administrations of other states, hostile to France but stunned by its success in mobilizing national resources, moved toward lasting reforms through what might be termed "defensive modernization." I shall here mention just three imposing examples: the British income tax, begun by William Pitt in 1799, revamped by Henry Addington in 1803; the establishment of the Prussian Ministry of War, the Great General Staff, and the Military Academy by Gerhard von Scharnhorst and his collaborators after 1807; and the creation in 1802 of central ministries even for stubborn Russia. However the governments of Europe may be characterized after the Congress of Vienna, they cannot be dismissed as mere survivals of the *ancien régime.*

A second set of changes took place in the nature of warfare, going far beyond the reorganization of military command functions and training, just mentioned in the case of Prussia. What happened over the twenty-three years between Valmy and Waterloo, the years of Marengo and Trafalgar, Austerlitz and Borodino, was a revolution in the demands, the implications, the very sociology of war. Nef, cited earlier as skeptical about a sharp and dramatic "Industrial Revolution," has no such misgivings about the explosive effect of combining the technological advances already achieved by the 1790's with the *levée en masse,* the harnessing of hordes of citizen-soldiers and their patriotic sentiments in the service of national aims. Here again, the continental enemies of France had to imitate or perish. By the time Napoleon was at last defeated, Europeans had experienced, if not total war, at least their first taste of what total war, including economic warfare, might be like under modern conditions.

The broadening and intensification of conflict were only the most obvious symptoms of a third type of change. This was the increased public involvement in politics. It was not just a matter of patriotism. Behind the sharp call of duty to the fatherland in danger, to the *patrie* as it was coming to be understood, could be heard a rising chorus of insistence on political rights for more citizens, though nowhere yet for all citizens. Palmer, in the study already noted, has shown that even before 1789 this insistence had been so clearly heard, and so strongly resisted by privileged groups, that the great crisis both inside and outside France was in part a "reaction against reaction." It was the Revolution itself, nevertheless, which mounted the most sweeping demands that, somehow, the people's representatives must be heard. Napoleon, while he subverted that demand, paid it lip service. After his fall,

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the restored dynasties compromised with it where necessary and condemned it where possible. If, however, it had subsided after 1815 like the winds of a passing storm, historians would not speak of the era of repression as essentially an episode. Instead, they could describe a lasting resumption or aristocratic "normalcy" drawn to the specifications of Friedrich von Gentz and Prince Metternich.

Still a fourth level of revolutionary change belongs to the realm of literature, music, and the visual arts. When dealing with the communication of sentiments, of course, we can scarcely expect to emerge with anything more tangible than an awareness of altered tone, an impression of changed intensity. It was just such an awareness, in this case one of lowered intensity, that struck R. H. Tawney some forty years ago, as his historical imagination moved out of the seventeenth century and its thunderous debates to contemplate their sequel. "In the great silence which fell when the Titans had turned to dust," he wrote, "in the Augustan calm of the eighteenth century, a voice was heard to observe that religious liberty was a considerable advantage, regarded 'merely in a commercial view.' "[11] How can we in turn, considering a later epoch, escape a powerful sense of change in just the opposite direction, of heightened emotional temperature, of art struggling to express newly unleashed passions when we contrast Pope with Shelley, Mozart with Berlioz (or even the Beethoven of the later symphonies), Goya of the court portraits and the tapestry cartoons with him of the "Horrors of War"? Must we not acknowledge that the differences represent long strides away from the sometimes cold confidence of the eighteenth century in *la raison raisonnante*? "And furthermore," as an elderly scholar of my acquaintance would say, with more petulance than precision, "things have never really calmed down since!"

Having said this much, let me now state my own conviction that nowhere —neither in administrative innovations, nor in the altered conditions of war, nor in the first uneven surge toward political democracy, nor in the highly charged cultural atmosphere—do we perceive at its clearest the fundamental shift that makes the last years of the eighteenth century and the first of the nineteenth a historical watershed too imposing to be disregarded. The most important change of all occurred in social structure and, equally important, in the way men conceived of social structure. We need above all to consider what it meant for European society to lose the appearance of a hierarchy of legally defined orders of men. By the same token, we must consider what it meant to have nakedly revealed the social subdivisions identified by Max

[11] R. H. Tawney, *Religion and the Rise of Capitalism* (New York, 1926), 197. The phrase is quoted from the British economist, Josiah Tucker.

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Weber in a famous and by no means outdated essay: classes, as economic groupings; status groups, reflecting degrees of honorific recognition; and parties, organized around shared political aspirations, if not always ideals or principles.[12]

It is worth noting in this connection that before 1789 social and political commentators labored under a double handicap in seeking to analyze the realities confronting them. In the first place, the cherished medieval vision of orders—noblemen, clergymen, burghers, peasants—seemed less and less meaningful, even when applied to Europeans who quite clearly, in a technical sense, did belong to nobility or clergy, bourgeoisie or peasantry. What real use, one might ask, was a category that covered English dukes, French lawyers, and the *glota,* the "barefoot gentry" of Poland, or Italian cardinals, Spanish friars, and poor German schoolmasters in clerical garb? What was the bourgeois brotherhood that united town patricians with the poor cobbler who stared hopefully at their shoes? What good did it do to call "peasants" both the independent *laboureur* on his French farm and the miserable worker on a Bohemian estate?

A second, still more serious trouble with the language of corps and orders was that it did not apply at all to large numbers of people who nonetheless deserved to be taken very seriously. The new Manchester textile manufacturer, the Genevan watch exporter only lately arrived from Basel or Lausanne, the immigrant Dutch wine merchant in Bordeaux were not legally "bourgeois" of their cities, but they most assuredly were businessmen. And what of the growing army of workers not accommodated by the guild system—to what order did they belong?

Let us not oversimplify. If it would be wrong to assume that European society before 1789 was in fact a tightly knit system of orders, it would be just as great an error to suppose that classes, status groups, and parties were as yet completely invisible. Quite the reverse was true, as we realize when we read bitter strictures concerning "the rich"—English nabobs back from India, French tax contractors, German speculators in grains. There was an old awareness too of "the poor" as a polyglot, urban-rural mass, an awareness born of countless seventeenth-century upheavals and of eighteenth-century troubles as recent as E. I. Pugachev's vast rebellion in Russia. If there were emergent classes, there were also eighteenth-century social elements whose honorific status escaped the traditional definition in terms of orders. We need here mention only the place occupied in most countries by the higher civil

[12] "Class, Status, Party," *From Max Weber: Essays in Sociology,* tr. and ed. H. H. Gerth and C. Wright Mills (New York, 1946), 180–95.

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service, the major bureaucrats, often humbly born and only moderately wealthy, whose power nevertheless excited both respect and animosity. And political parties, as Palmer has shown,[13] can be identified with relative ease by the 1780's: "Patriots" and "Orangists" in the United Provinces, "Vonckists" and the "Estates Party" in the Austrian Netherlands, "Republicans," "Moderates," and "Patriots" in beleaguered Poland.

Yet the language of orders hung on until the Great Revolution, and in so far as that language expressed the accepted, the respectable way of describing human relationships, it was itself a conditioning factor in the situation. For while Europe's population was not, in fact had never been, neatly subdivided into nobility, clergy, bourgeoisie, peasantry, and certain less general orders, a conventional terminology suggesting that those were the only meaningful rubrics still helped to shape men's reactions to developments. Edmund Burke, like Montesquieu before him, both pleaded for reform and denounced revolution in the name of healthy relations among stable ranks of men. Gaspar Melchor de Jovellanos, gifted and enlightened though he was, saw fit to dramatize Spain's ills in 1787 by composing a poem on the sad decline of the ancient nobility.[14] In Württemberg and in other German states, the critics of princely absolutism scarcely went beyond a reassertion of the rights of the legal orders, the *Stände*.[15] Even in the early polemical writings of the French Revolution, notably including those of the Abbé Sieyès, we perceive the hold that old terms and categories had maintained on the political imagination of the day. The confused vehemence of the 1780's, the impression of issues badly joined, not only in France but in other countries as well, seems to me to testify to the inadequacy of an inherited conceptual scheme when applied to recalcitrant circumstances.

It was the pitiless test of power imposed upon most of Europe by the revolutionary-Napoleonic crisis that killed the old image of society—not completely nor all at once, to be sure. Vestiges of archaic language and values have survived to the present day, sometimes twisted to more modern polemical uses. (Incidentally, when we label "bourgeois" a suburban ranch house, with a television antenna above and a two-car garage on the side, do we speak with the scorn of noblemen, the pride of burghers, the envy of peasants, or the righteous wrath of proletarians?) In any case, the false symmetry of a single hierarchy of orders never recovered from the shock it received in the quarter century that opened in 1789.

[13] Palmer, *Age of the Democratic Revolution*, Chaps. XI and XIII.

[14] *La satire de Jovellanos*, ed. Alfred Morel-Fatio (Bordeaux, 1899); cited by Richard Herr, "The Twentieth Century Spaniard Views the Spanish Enlightenment," *Hispania*, XLV (No. 2, 1962), 185.

[15] F. L. Carsten, *Princes and Parliaments in Germany from the Fifteenth to the Eighteenth Century* (Oxford, Eng., 1959).

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That the Revolution contained elements of class conflict, in its most precise, economic sense, was apparent even before the Estates-General came together at Versailles. The *Affaire Reveillon* of April 1789, for instance, was a bloody riot touched off by the efforts of two wealthy Parisian manufacturers (members of the Third Estate) to impose lower wage scales. It is my own belief that Marxist historians, from Albert Mathiez to Albert Soboul,[16] have sought to make class conflict explain more facets of the French Revolution than it can in fact account for. It would be foolish, however, to deny that the struggle between poor men and those more comfortable runs back and forth through the historical fabric of the period, from the Faubourg Saint-Antoine to German cities on the Rhine, from the streets of Amsterdam to the shimmering water front of Naples.

At the same time, we also encounter intensified party strife, the competition of political groups for power as an end in itself. The succession of such groups in the French assemblies—Feuillants, Girondists, *Montagnards, Hébertistes*—is so familiar that it can easily be underrated as a new chapter in parliamentary history. No less significant, however, was the effect of events in France on other countries, the tendency in one government after another for "anti-" and "pro-French" parties to appear under such conditions as the local situation offered. In England, for example, war fever and the dread of Jacobin excesses combined to discredit Charles James Fox in the 1790's, but not before his collision with Pitt had given a new sharpness to the struggle between Whigs and Tories.

Gradations of social status, no less than economic and political alignments, were deeply affected by the upheaval. In France, and in other lands that experienced even a forced transplantation of the Revolution, a decadelong assault both on ecclesiastical independence and on the privileges of birth left the honorific position of clergy and nobility—not to mention their physical base of power, especially in land—damaged beyond hope of complete repair. Admittedly, neither order was destroyed, but henceforth neither could look down with secure disdain on the rest of society. The revolutionaries' glorification of "citizen" as the proudest of all titles, like their insistence that love of country outweighed humble birth, struck at the very roots of inherited rank and at the mystical awe surrounding prelates.

In place of the old determinants of status, certain others gained a degree of general acceptance that they have commanded ever since. I refer to wealth, special abilities, and service to the community (especially if recognized by the bestowal of public office or military rank). Questions might still be asked

[16] See esp. Albert Soboul, "Classes and Class Conflicts during the French Revolution," *Science and Society*, XVII (No. 3, 1953), 238–57, and *Les Sans-culottes parisiens en l'an II* (Paris, 1958).

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about an individual's family tree and his way of life, but they seemed less and less relevant when compared with other, more urgent queries: Is he rich or poor? What can he do? What is his present position? The modern status system may seem no more attractive than that of the *ancien régime,* but the differences between the two are unmistakable.

The Imperial Nobility established by Napoleon in 1808 provides, somewhat surprisingly, an excellent example of basic change. To a superficial observer it might appear that the Emperor was simply resurrecting aristocratic privilege, as underpinning for his dynasty. But let us not overlook two features of this new hierarchy. First, its titles were assigned on the basis of military services rendered or public offices already held by the recipients. Ministers and senators became counts; presidents of departmental electoral colleges, higher judges, mayors of the larger cities became barons; members of the Legion of Honor became chevaliers. Second, such a title, even after having been conferred on an individual, could pass to his descendants only if accompanied by a fortune sufficient to support it. To meet this requirement, a prince of the Empire would have to bequeath an estate yielding at least 200,000 francs per year, while the corresponding figure for a count was 30,000, for a baron 15,000, and for a chevalier 3,000.[17] Napoleon visualized not only a nobility of service, but also one that would remain an "upper class" in specifically economic terms. In his shrewd, cynical mind and in the minds of many of his contemporaries there remained no room for arguments about the virtues of aristocratic birth or leisured refinement or genteel poverty.

I have asked for due consideration of a series of institutional, military, political, and aesthetic changes and, in slightly more detail, a set of complex but crucial changes in social structure. Any one of these factors might in itself be dismissed as unrepresentative, or at most a matter only of degree. Taken together, however, they reveal a revolution in the fullest sense, a fundamental departure from some of the most important conditions of human life before 1789. The magnitude and the nature of this phenomenon will surely escape the historian whose gaze is riveted on just one country or on only one type of evidence, be it diplomatic correspondence, official enactments, personal reminiscences, or belles-lettres. But the historian who is willing to look up, however briefly, from his specialized labors and to indulge in a panoramic view can scarcely avoid the impression of looking back toward a massive divide, a true watershed.

To recognize such a divide is not to ignore all the important threads of continuity mentioned at this essay's beginning. I do not, for example, sup-

[17] Félix Ponteil, *Napoléon I^er^ et l'organisation autoritaire de la France* (Paris, 1956), 124.

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port the claim that the men of the old regime are more remote from us than St. Thomas Aquinas was from them. On the other hand, there is little to be gained and much to be lost by overlooking the watershed of the Revolution or by underestimating the effort required for us to think our way back across it into the eighteenth century. In 1789, after long, confused preliminaries, the old Europe began a transformation, convulsive, bewildering, to some of the participants wildly exhilarating, to others bitterly tragic. In 1914 another convulsion began. Its successive spasms were destined to last even longer than those of its predecessor. Its human cost was far more terrible, and it contained much less that was either generous or hopeful. Perhaps the greatest tribute that can now be paid to European civilization, to its vitality, its adaptability, and its tenacity, lies in the observation that through two such cataclysms we can still trace so many familiar lines, truly uniting the centuries. Fully aware of those lines, historians should not shrink from marking the changes along the way.

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Ancient Egypt: A Survey of Current Historiography

MIRIAM LICHTHEIM *

BORN in Paris on September 27, 1822—the day on which J. F. Champollion's famed *Lettre à M. Dacier relative à l'alphabet des hiéroglyphes phonétiques* was read at the *Académie des Inscriptions et Belles-Lettres*—Egyptology is among the youthful historical disciplines, and the nineteenth century is its heroic age. Following on Champollion's decipherment of hieroglyphic writing, scholars of the Western world streamed to Egypt to conduct large-scale surveys of the extant monuments, subsequently published in magnificent folios of abiding worth.[1] The excavation work, which came after the surveys and gained momentum throughout the second half of the century, was at first often ill-conceived and poorly executed and was little better than the treasure-hunting carried out by amateurs. But though one must deplore the hasty removal of monuments without proper recording, it is only fair to say that through their removal to the museums of the Western world these objects have escaped destruction. For the natives of Egypt were all the while engaged in annihilating the monuments, seeing in the stones an excellent source of lime and building materials.[2] The marble of Roman buildings was especially prized for conversion into cement; thus these buildings, many of which had still been seen and sketched by scholars conducting surveys in the middle of the century, have almost completely disappeared. And much of the modern city of Alexandria was built by dismantling the ruins of the classical city and reusing its stones as foundations. Even worse, a large proportion of

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[1] E. g., J. F. Champollion, *Monuments de l'Égypte et de la Nubie* (4 vols., Paris, 1835–45); Richard Lepsius, *Denkmaeler aus Aegypten und Aethiopien* (12 vols., Berlin, 1849–56) and *Text,* ed. Edouard Naville (5 vols., Leipzig, 1897–1913).

[2] Another major cause of destruction was illicit digging and cutting up of pieces for sale to tourists. Travelers from the West have furnished many accounts of this practice. The following quotations will serve to illustrate the principal methods of destruction: "I went up . . . to search for the nineteen tombs which Lepsius copies from in the *Denkmaeler*. . . . These tombs were blasted out when the sugar mills were built; they are in good limestone." (Charles Edwin Wilbour, *Travels in Egypt, December 1880 to May 1891* [Brooklyn, N. Y., 1936], 453.) "We went up and saw the ruins of the Colossus tomb behind Bersheh. Sayce who saw it last year did not wish to go into it again. It is sickening. Great portions are cut away and all the rest hammered except two strips about two feet wide which having been exposed to a fall of dust are so faded that the marauders did not think them worth destroying." (*Ibid.,* 582–83.)

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the fragile papyrus documents that came to light in illicit digging were destroyed in a variety of ways, and with them went the chance ever to reconstruct Egypt's ancient civilization in any but a sketchy and lopsided manner.

In the last three decades of the nineteenth century decisive steps to put the new science on a firm basis were taken. Scientific excavation, observing proper standards and techniques, became the rule everywhere. Simultaneously, the study of ancient Egypt's several scripts—hieroglyphic, hieratic, and demotic —had reached maturity, and, with reading difficulties removed, the grammatical works that laid bare the structure of the language in its successive stages from old Egyptian to Coptic were being published, not without impassioned verbal battles between scholars of different nations holding widely divergent views. When the battles were over, Egyptology possessed a firm body of grammatical works, a number of exemplary text editions, a sound technique of excavation and recording, and vast amounts of material stored in museums.

Equipped with this inheritance, scholars of the twentieth century of necessity had to become specialists. The quantities of material are too great, and the differences between linguistic, archaeological, and historical studies too fundamental to allow any one scholar to encompass the whole field. In the words of Sir Alan Gardiner, "the rivulet of Egyptological research was gradually swelling into the mighty stream which now makes it impossible for any student to keep abreast of all that is written save at the cost of abandoning all hope of personal contributions."[3] Specialization, then, is the rule. It includes the plodding investigation of minutiae as well as the painstaking reinvestigation of acquired beliefs which, firmly enshrined in books, have become doctrines. It also includes the ceaseless reinterpretation of Egyptian history.

The writing and rewriting of ancient Egyptian history are the topics of this article. The rewriting of history by successive generations is obviously the result both of specialization and of its corollary: the desire to synthesize and to achieve an integrated view. Just as the amount of specialized research is ever growing, so the general histories of Egypt that attempt an over-all synthesis are becoming more numerous. Before speaking of the general history of Egypt, the student may look at its major components: the main periods of Egyptian history and the monographs that sum them up.

Prehistoric Egypt is still largely an enigma. The body of material evidence is comparatively small and tends to sag under the weight of speculation built

[3] A. H. Gardiner, *Egypt of the Pharaohs* (Oxford, Eng., 1961), 16. The *Annual Egyptological Bibliography*, compiled by the indefatigable Pater J. Janssen since 1946, lists an average of six hundred books and articles.

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upon it. Who were the people that created the prehistoric cultures of the Nile Valley? An African people speaking a Hamitic language subsequently fused with a Semitic-speaking group? This is the prevailing but by no means firmly established view. And who were these immigrating Semites? By what route did they reach Egypt? What of the material differences between the Upper and Lower Egyptian prehistoric cultures, which have been construed as a contrast between seminomadic hunters and sedentary farmers? Only one thing is certain: in historical times, the contrast between Upper and Lower Egypt was a psychological reality to which the ancient Egyptians gave frequent expression. But the actual basis of this contrast, other than the obvious geographic difference between the two halves, is unknown to us. What factors had entered into it remains in the realm of speculation until such a time as significant new evidence is discovered. In any event, the prehistoric division of the country came to an end with the unification of the land under the first kings of the First Dynasty, a unification that was preceded by warfare and was achieved through the victory of the South over the North.[4]

In exchanging the quicksands of prehistory for the firmer ground of historic Egypt under the First Dynasty, one is confronted with a basic problem, one of those fundamental questions that it is more fruitful to ask than to answer, namely, the problem of the seemingly sudden birth, or rise, of the first high civilizations produced by man—the ancient Egyptian and ancient Mesopotamian—and the how and why of the origin of civilization itself. No doubt, Arnold J. Toynbee's monumental attempt to schematize historical causation has done much to stimulate debate of this topic, and a number of significant publications have resulted. For the present purpose the most pertinent of these is the volume of papers presented at a symposium at the University of Chicago in December 1958.[5] In the words of its preface, "the purpose of this symposium was to explore the circumstances under which in the Near East man for the first time in human history attained those higher levels of cultural life that we associate with the word 'civilized.'" The participants considered the problem from many angles, and numerous fruitful ideas were expounded. Mesopotamia appeared to yield more readily to the key definitions of emerging civilization that were offered. Egypt proved recalcitrant. "The abrupt emergence of the Egyptian nation at about 3000 B.C.

[4] The most recent work on Egypt's earliest times—prehistory, unification, and the first two dynasties—by a scholar whose highly successful excavation work makes him eminently qualified to speak of this period is W. B. Emery, *Archaic Egypt* (Harmondsworth, Eng., 1961). Other works are conveniently listed in the excellent bibliography of W. C. Hayes, *The Scepter of Egypt: A Background for the Study of the Egyptian Antiquities in the Metropolitan Museum of Art* (2 vols., Cambridge, Mass., 1953–59), I, 357–58.

[5] *City Invincible: A Symposium on Urbanization and Cultural Development in the Ancient Near East,* ed. Carl H. Kraeling and Robert M. Adams (Chicago, 1960).

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is a terrible embarrassment." [6] In descriptive terms we can of course say much about this emergence and its characteristic features. It was done incisively by the late Henri Frankfort in a study that compared the civilizations of Mesopotamia and Egypt in their early stages.[7] It is no accident that Frankfort devoted an entire chapter to a refutation of Toynbee's views. For the challenge and response formula cannot account for the rise and growth of any one of the historic civilizations. The formula is devoid of content and cannot describe, much less explain, the individual character of a civilization. In Frankfort's words: "In studying the birth of a civilization we are concerned with the emergence of its 'form.'" [8] Parallel conclusions were formulated by a group of German scholars who lectured on the rise and fall of the civilizations of the ancient Near East.[9] Discussing Egypt, Walther Wolf stressed the inexplicability of the phenomenon of the birth of civilization.[10]

About 3000 B.C., then, the people of Egypt made the great leap forward into civilization through the simultaneous creation of several of its basic features: the invention of writing; monumental art; and strong, centralized political organization, namely, the divine monarchy that had extended its rule over the population of the whole Nile Valley from the first cataract to the Mediterranean.

From here on, Egyptian history is outlined through the sequence of thirty-one dynasties of kings, a chronological framework that has been worked out on the basis of the ancient sources themselves. Correlations with the other civilizations of the ancient Near East have established additional points of synchronic chronology.[11] While the framework is firm, many details are uncertain and subject to revision. The dates for the first eleven dynasties are still

[6] Quoted from John A. Wilson, *ibid.*, 130.

[7] Henri Frankfort, *The Birth of Civilization in the Near East* (Bloomington, Ind., 1951; paperback reprint, 1956).

[8] *Ibid.*, 31. This essential form, the individual character of a civilization that has reached maturity, had been the topic of an earlier collective effort at the University of Chicago: Henri Frankfort *et al.*, *The Intellectual Adventure of Ancient Man: An Essay on Speculative Thought in the Ancient Near East* (Chicago, 1946). It was reprinted without the chapter on the Hebrews as a Pelican paperback under the title *Before Philosophy* (Harmondsworth, Eng., 1949; reprint, 1959). Here, the modes of thought of Egypt, Mesopotamia, and Israel were described in the round, and the mythmaking quality of their thinking was contrasted with rational Greek thought. *Intellectual Adventure* is an outstanding book and the best introduction to the world views of the ancient Near East.

[9] *Aufstieg und Untergang der Grossreiche des Altertums*, ed. W. F. Mueller (Stuttgart, 1958).

[10] *Ibid.*, 17: "Wir glauben in der Tat, dass wir gut daran tun, uns hier zu bescheiden und in der Geburt der ägyptischen Kultur—wie jeder anderen—ein Mysterium zu sehen, das wir mit Ehrfurcht betrachten, aber nicht als Wirkung irgendwelcher Ursachen zu 'erklären' versuchen sollten." Yet the Toynbee oriented desire to explain the rise of civilization continues to have thoughtful adherents, e.g., Rushton Coulborn, *The Origin of Civilized Societies* (Princeton, N. J., 1959).

[11] See *Relative Chronologies in Old World Archeology*, ed. R. W. Ehrich (Chicago, 1954).

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variable within a range of approximately three hundred years, depending on the date one chooses for the beginning of the First Dynasty and on the manner of calculating the lengths of the gaps within the dynasties. From the Twelfth Dynasty on there are a number of firmly fixed dates, and the margin of uncertainty grows progressively smaller. In any event, the date for the beginning of the First Dynasty is no longer set any higher than 3200 B.C. In his *Egypt of the Pharaohs* Gardiner devotes considerable space to chronological problems, and, in his king-list, omits all absolute dates before the Twelfth Dynasty, thus giving a much clearer picture than is usually provided of what is known and what is conjectural.

While the study of chronological problems leads to slow revision, the discussion of Egypt's calendar, or more accurately, calendars, has been revolutionized through R. A. Parker's *The Calendars of Egypt* (Chicago, 1950). All earlier works on the subject are obsolete.

Now whereas the frame of thirty-one dynasties is rooted in ancient Egyptian traditions, the additional division into major periods—Old Kingdom, Middle Kingdom, New Kingdom (or Empire period), and Late Period—was developed in the nineteenth century and represents the modern over-all view of the course of Egyptian history. For the time being, this scheme seems to be an adequate description of what to us moderns appear to be major phases of development of Egyptian history, each significantly different from the others. It is nevertheless possible that the terminology will undergo modification. In particular, the term Late Period, being almost meaningless, is not an adequate designation for the very complex history of Egypt from the end of the New Kingdom to the arrival of Alexander the Great (1080–332). Nor is the alternate label, namely, Period of Decline, any more fortunate. Of this more will be said later. There is, of course, always the danger that, once labeled, these periods acquire a stereotyped image; they are also easily misused to prove a point. Thus, in Toynbee's treatment, the Old Kingdom alone stands out as a period of creativity, while all subsequent periods are seen as mere phases of stagnation illustrating the "laws" of cultural decline, to my mind a completely spurious reading of Egyptian history. The division into periods, then, is designed to emphasize that in the course of its long history Egyptian civilization attained three peaks of political strength and cultural florescence, each distinctly different from the other and separated from one another by two phases of disorder and disintegration, known as the First and Second Intermediate Periods. It is only natural that most scholarly work is devoted to these three peaks: the Old, Middle, and New Kingdoms. Yet future work will undoubtedly pay more attention to the much-

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neglected Late Period, and, it may be hoped, even to its sequel, the centuries of Ptolemaic and Roman rule, though these are usually considered to lie outside the Egyptologist's ken and to belong to classical scholarship.

Mention should be made of a few works that treat significant aspects, or phases, of Egyptian history from the beginning of the Old to the end of the New Kingdom. First of all there is the admirable monograph on the pyramids by I. E. S. Edwards.[12] The same ground is covered in more popular form by Ahmed Fakhry.[13] Toward a history of the Middle Kingdom we have the close scrutiny of its archaeological sources by H. E. Winlock.[14] The Middle Kingdom is Egypt's "classical" age. The archaic simplicity of the Old Kingdom had given way to self-consciousness, sophistication, and complexity. Art works were both powerful and elegant. Literature had attained a mature variety of forms and a literary style that was to remain normative.[15] The portrait statues of the kings of the Twelfth Dynasty are the complete embodiment of a perfected ideal of sculptural expression. They possess the same finality as the works of Periclean Athens. Indeed, the Middle Kingdom is in the history of Egypt what fifth-century Athens is in the history of Greece: a brief and tenuous moment of perfection.

The times of trouble known as the First and Second Intermediate Periods (before and after the Middle Kingdom respectively) have been investigated a number of times and are emerging in revised form. The First Intermediate no longer seems quite so chaotic or revolutionary. True, the authority of the divine monarchy had broken down, and from the viewpoint of national unity this was a disaster. But the country was governed quite effectively by the provincial rulers (nomarchs), who had evolved from royal appointees to independent princes and were exercising complete control over their territories. The Second Intermediate Period, too, now appears as a time of gradual transitions rather than abrupt and chaotic changes. It is now generally thought that the Hyksos were not a foreign nation of unknown origin that invaded Egypt en masse but that they were Palestinian petty chiefs in search of new lands who gradually infiltrated the Delta.[16] As they gained strength,

[12] I. E. S. Edwards, *The Pyramids of Egypt* (Harmondsworth, Eng., 1947; 2d ed., London, 1961).

[13] Ahmed Fakhry, *The Pyramids* (Chicago, 1961). Furthermore, a definitive study of the pyramids has now begun to appear: J. P. Lauer, *Histoire Monumentale des Pyramides d'Égypte,* Institut français d'archéologie orientale, Bibliothèque d'étude, XXXIX (Cairo, 1962–). To date Volume I in two parts has been published.

[14] H. E. Winlock, *The Rise and Fall of the Middle Kingdom in Thebes* (New York, 1947).

[15] A highly original and pioneering study of the political aspects of some literary works of the Twelfth Dynasty is Georges Posener, *Littérature et politique dans l'Égypte de la* XII^e *dynastie* (Paris, 1956). Middle Kingdom sculpture is impressively presented in H. G. Evers, *Staat aus dem Stein* (2 vols., Munich, 1929).

[16] Cf. Albrecht Alt, *Die Herkunft der Hyksos in neuer Sicht,* Sitzungsber. der Sächsischen

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they set themselves up as kings in Lower Egypt and, not having a significant culture of their own, adopted Egyptian names and manners. The extension of their rule into Upper Egypt was of short duration and was followed by their complete expulsion from Egypt. This swift and dramatic event ushered in an entirely new situation: the reunification of Egypt under the powerful, militant kings of the Eighteenth Dynasty and their deep thrusts into Western Asia, which created the "empire."

The New Kingdom, Egypt's age of splendor, is the best documented period of Pharaonic history.[17] Its numerous literary sources and countless works of art show this ancient nation at the peak of its power and wealth; they also reveal a profoundly altered view of life. If the culture of the Old Kingdom was archaic, primitive, secure, isolationist, and rooted in unquestioned beliefs, the culture of the New Kingdom was typically modern: outward-looking, cosmopolitan, sophisticated, refined, restless, and searching. This change, the beginnings of which were already apparent in the Middle Kingdom, is so striking and can be traced so clearly in all manifestations of Egyptian civilization—language, literature, art, religion—that one wonders how the misguided notion of an unchanging, undynamic, Pharaonic culture, which still haunts some histories of civilization, could ever have obtained credence.[18]

The new sophistication and restlessness are also the setting for the drastic religious and artistic reforms of King Amenhotep IV (Akhenaten) known as "the Amarna revolution." No other episode in Egyptian history has been the object of so much discussion. The volume of source material is small and does not permit a complete reconstruction of events.[19] All the more is it necessary to construct a precise account of the actual events, to the extent that the sources allow, and to keep one's speculation on the meaning of the events in a separate compartment. Such a factual and masterly account can now be found in Gardiner's *Egypt of the Pharaohs,* Chapter IX. Such current guesswork as Akhenaten's alleged attempt at conciliation with Thebes to-

Akad. d. Wiss., Phil.-hist. Kl., 101/6 (Berlin, 1954). Torgny Säve-Söderbergh, "The Hyksos Rule in Egypt," *Journal of Egyptian Archaeology,* XXXVII (1951), 53–71.

[17] Some of its dynamism, radiance, and luxury is conveyed in George Steindorff and K. C. Seele, *When Egypt Ruled the East* (Chicago, 1947; 2d ed., 1957).

[18] These general histories of civilization are the bane of college teaching. Mostly written by nonspecialists and compressing centuries or millenniums of cultural development into a few pages, they abound in distorting oversimplifications, unwarranted generalizations, and plain errors. Nothing could be less successful in introducing students to history than this type of book.

[19] The principal sources are found in: N. de G. Davies, *The Rock Tombs of el Amarna* (6 vols., London, 1903–1908); T. E. Peet *et al., The City of Akhenaten* (4 vols., London, 1923–51); *The Mural Painting of El 'Amarneh,* ed. Henri Frankfort (London, 1929). For further bibliography, see Hayes, *Scepter of Egypt,* II, 446–48.

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ward the end of his life, the supposed peace mission of Smenkhkare, and the "fall from favor" of Queen Nefertiti gains little support from Gardiner's restrained summation. Egyptian textual sources are always allusive rather than explicit, and the archaeological evidence, such as tomb equipment, if it has survived at all, is frequently out of context owing to reuse. Thus the existing source material will simply not answer our inquiries into the critical stages of the movement. Both the beginnings of Atonism, while Akhenaten was still at Thebes, and its end, that is, the circumstances under which the King died and the movement collapsed, are shrouded in darkness. Only the middle span, the decade of life at El Amarna, is fairly well documented. Here the sources, excavated on the site of the ruined capital, convey a vivid picture of the way of life at Amarna, centered around the royal family's worship of the Sun. Some recent studies tend to emphasize that Akhenaten's new religion of the Sun was not altogether new because it harked back to the old sun worship of Heliopolis, and because its universalism was already expressed in the hymns to Amon which precede his famous hymn to the Aton.[20] Atonism in its fully developed form was, nevertheless, a complete break with the past, and nothing proves this more than the hatred and vigor with which all traces of it were wiped out after its collapse. What then was the essence of the new worship of the sun disk? Was it a true monotheism? This has often been affirmed and is nowadays sometimes denied.[21] Under whatever label one sums it up, Atonism remains a truly revolutionary break with traditional Egyptian religion, a break that affected the whole manner of life and its expression in art. To all who were not converted to it, Atonism was a profound affront. And the speed with which the movement collapsed indicates that it had not struck root among the people and that the conservative forces aligned against it represented the country at large.

Nevertheless, though Atonism was defeated, modernism was not. The reinstatement of the pantheon and the restoration of traditional worship could not bring about a return to the simplicity and certainty of earlier centuries. The spirit of restlessness and the questioning of inherited modes of life remained.

[20] Cf. John A. Wilson, *The Culture of Ancient Egypt* (Chicago, 1951), Chap. IX, esp. 207–12. This was originally published in hardback as *The Burden of Egypt* (Chicago, 1951). The paperback edition's latest printing has the welcome addition of a map. A complete king-list would be another much-needed addition.

[21] Notable defenders of the view that Atonism is monotheism are many. James Henry Breasted claimed it fifty years ago, and it is now upheld by Gardiner (*Egypt of the Pharaohs*, 227). The view that Atonism falls short of monotheism in several essential respects is developed by Wilson in *Culture of Ancient Egypt*, Chap. IX. Siegfried Morenz, *Ägyptische Religion* (Stuttgart, 1960), 154–55, sees in Akhenaten's doctrine a form of trinitarianism, embracing the triad Re-Shu-Aton. This new interpretation has not yet been widely discussed.

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Even more than the Eighteenth Dynasty, the Ramesside age (Nineteenth–Twentieth Dynasty) despite its efforts at restoration was an age of restlessness. Everywhere in the Near East it was an age of change and of large-scale movements of new nations. The vigorous and soldierly Ramesside kings were hectically busy defending Egypt's foreign possessions, and the homeland itself, against successive waves of foreign peoples. When the wars against the Hittites ended in a draw, a peace treaty resulted which is a document of amazing modernity.[22] The intellectual mobility of Ramesside times can be gleaned from all types of its literature, from schoolboy compositions, to bawdy folk tales, to mortuary inscriptions. Skepticism, that is, the questioning, or outright denial, of traditional beliefs, is a recurring theme. There are a liking for foreign fashions, a satirical humor, and growing awareness that the approach to the divine powers is not wholly through the community but that the individual stands in a direct personal relationship to them.[23] These and other features of Ramesside culture have sometimes been interpreted as signaling the loss of the old equilibrium and hence the beginning of decline.[24] It is true that after 1100 B. C. Egypt did decline, at least in a political sense. It had lost its foreign possessions and its access to foreign markets, and it was threatened, and ultimately invaded, by newly rising nations and empires. But the view that sees this loss of political power foreshadowed in the intellectual insecurity and restlessness of the preceding Ramesside age, interpreting this restlessness as a loss of the old certainties and hence a beginning dissolution of the nation's fabric, is based on the presupposition that ancient Egypt had achieved its complete cultural expression during the Old and Middle Kingdoms and that therefore the complex tendencies of the New Kingdom can have only one sense: they are the symptoms of a beginning dissolution and decline. But the intellectual ferment of the New Kingdom need not be construed as a symptom of decline. Instead, the de-

[22] It includes such features as a nonaggression pact combined with a defensive alliance, and an agreement on mutual extradition of fugitives. For an English translation of the text, see *Ancient Near Eastern Texts Relating to the Old Testament,* ed. James B. Pritchard (Princeton, N. J., 1950; 2d ed., 1955), 199–203.

[23] Cf. Battiscombe Gunn, "The Religion of the Poor in Ancient Egypt," *Journal of Egyptian Archaeology,* III (1916), 81–94.

[24] E.g., Wilson, *Culture of Ancient Egypt,* 242: "It would seem that the word of the king no longer had the same effectiveness in maintaining order; pharaoh was no longer respected and feared as he had been in the more sacred state of earlier times." *Ibid.,* 243: "Magic had always been an element of Egyptian life. . . . This later period, however, showed an increased reliance upon various magical techniques and powers. Insecurity brought a longing for greater protection through some kind of external potency. . . . Man was no longer strong enough in himself." *Ibid.,* 267: "The treatment of the gods [in Ramesside tales] as subject to human foibles and weaknesses was not new in Egypt, but the broadness of this treatment in the Late Empire suggests that the sacred was no longer held in the same reverence. The supporting post of ancient Egyptian culture was showing visible cracks. If nothing could be taken with complete seriousness, what would hold society together?"

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partures from the old norm can be seen as part of a continual process of change, which was at work in all stages of Pharaonic culture and which led to transformations that were well within the nation's potentialities of growth. The difficulties that beset Egypt at the end of the New Kingdom were due to factors beyond its control; the resulting centuries of struggle have been prematurely lumped together as "Period of Decline." Because all standard histories of Egypt treat this Late Period (1080–332 B. C.) very cursorily or not at all, I shall, at this point, briefly survey the general histories and shall leave the discussion of the Late Period and its Greco-Roman sequel to the end.

The best of the general histories of ancient Egypt is a French work.[25] A narrative history with extensive bibliographic annotations, it addresses itself to specialist and intelligent general reader alike. It has no counterpart in English. Half a century has passed since James Henry Breasted wrote his *History of Egypt*. It was an excellent work in its day, and its passing is regrettable. Yet it is so completely out of date that it can no longer be used. Its place has now been taken by two recent works, which have already been mentioned several times: *The Culture of Ancient Egypt* by Professor John A. Wilson,[26] and Gardiner's *Egypt of the Pharaohs*. Both have many admirable features and lend themselves well to use as college textbooks. They are, moreover, in many ways complementary, Wilson's being more cultural than political history, Gardiner's more political with emphasis on problems of chronology. Being the work of outstanding scholars, both are representative of present-day scholarship, which is not to say that their judgments are indicative of any consensus of opinion. The very tentative nature of all writing on ancient Egypt has already been emphasized, and Gardiner's book is deliberately cautious. By contrast, Wilson has chosen to underline the hazards of interpretation by a very free use of subjective evaluations which are clearly identified as personal opinions, and with which he challenges the reader. Despite the contrast in method, the basic awareness is the same; it is expressed in Gardiner's warning: "It must never be forgotten that we are dealing with a civilization thousands of years old and one of which only tiny remnants have survived. What is proudly advertised as Egyptian history is merely a collection of rags and tatters."[27] This awareness of limitations seems to prevail to a greater degree today than it did fifty years ago. We no longer write as if we knew what went on in the mind of Akhenaten. Inevitably, this

[25] Étienne Drioton and Jacques Vandier, *L'Égypte*, Les Peuples de l'Orient Méditerranéen, II (Paris, 1938; 2d ed., 1946; 3d ed., 1952; 4th ed., 1962).

[26] See note 20, above.

[27] Gardiner, *Egypt of the Pharaohs*, 53.

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mood of caution does not come out so clearly in the popular histories. These, by their very nature, are bound to simplify, and through simplification they create the illusion that Egyptian history is a dense fabric of facts and not the "collection of rags and tatters," filled out by speculation, which it actually is.[28]

Histories are indispensable. Yet it would seem that the present state of our knowledge is more successfully encompassed by handbooks, and several excellent ones have appeared in recent years. *Ancient Egypt*[29] and *The Scepter of Egypt*[30] are both museum publications that combine the functions of museum guides with those of cultural histories and succeed extremely well in presenting an abundance of material in attractive and readable form. Here also belong the recent *Dictionary of Egyptian Civilization*[31] and the *Reallexikon der ägyptischen Religionsgeschichte*.[32] Mention should also be made of the very useful *Ancient Egypt, A Cultural Topography*.[33]

The Egyptian chapters of the new edition of the *Cambridge Ancient History* will undoubtedly serve as basic text for the political history of Egypt for some time to come. Future one-man histories, to venture a forecast, will attempt to focus on social and economic factors, the study of which is still in its infancy. It is obvious that the peculiar character of the source material—the abundance of religious and ceremonial inscriptions and of works of art, the comparative paucity of documents of daily life, and the highly stylized, allusive, and unexplicit character of all literary records—has dictated the course of Egyptian historiography. Political history, art, and religion have been the central topics; the study of social and economic factors, handicapped as it is by the nature of the material, requires refined methods and much closer scrutiny of the sources. A few pioneering studies have appeared, and this, undoubtedly, is the most fruitful area of future research.[34]

[28] Within these limitations, Cyril Aldred, *The Egyptians* (London, 1961), is an excellent, brief, illustrated, popular history.

[29] William S. Smith, *Ancient Egypt as Represented in the Museum of Fine Arts* (Boston, 1942; 3d ed., 1952; 4th ed., 1960).

[30] See note 4, above.

[31] *A Dictionary of Egyptian Civilization*, ed. Georges Posener (London, 1962). The original French edition, *Dictionnaire de la civilisation égyptienne*, was published in Paris in 1959.

[32] Hans Bonnet, *Reallexikon der ägyptischen Religionsgeschichte* (Berlin, 1952).

[33] Hermann Kees, *Ancient Egypt: A Cultural Topography* (Chicago, 1961). The original German edition, *Das alte Ägypten: Eine kleine Landeskunde*, was published in Berlin in 1958. For the specialist, and on a larger scale, is Pierre Montet, *Géographie de l'Égypte ancienne* (2 vols., Paris, 1957–61). While this is not a survey of handbooks, two more basic works of reference might be mentioned: Jacques Vandier, *Manuel d'archéologie égyptienne* (3 vols., Paris, 1952–58); *Handbuch der Orientalistik*, ed. Bertold Spuler (Leiden, 1952–), I, *Ägyptologie*. Our rather scanty knowledge of Egyptian law is summed up by Erwin Seidl, *Einführung in die ägyptische Rechtsgeschichte* (Glückstadt, 1951).

[34] E.g., Jaroslav Černý, "Prices and Wages in Egypt in the Ramesside Period," *Cahiers d'histoire mondiale*, I (1954), 903–21; Wolfgang Helck, "Die soziale Schichtung des ägyptischen Volkes im 3. und 2. Jahrtausend v. Chr.," *Journal of the Economic and Social History of the Orient*, II (1959), 1–36. As this survey is confined to history proper, works on Egyptian re-

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It seems fitting to conclude this roundup of general works by hailing the appearance of a handsomely illustrated comprehensive study that traces the Western world's view of ancient Egypt from classical antiquity to the end of the eighteenth century.[35] From classical times on, the West was fascinated by what it believed to be Egypt's ancient and mysterious wisdom, a wisdom penetrating to the essence of things and embodied in hieroglyphic signs. Thus, as hieroglyphic inscriptions could no longer be read, there grew up a tradition of allegorical interpretation of hieroglyphic signs that yielded fantastic and marvelous results. This book tells the whole fascinating tale and ends fittingly with a dramatic account of the decipherment of hieroglyphs, in which several scholars played a part, but which was accomplished solely by the immensely brilliant, and tragic, Jean François Champollion.[36]

ligion, literature, and other topics have not been mentioned. Yet it might be useful to refer to two recent articles that survey publications on some of these topics: Thomas O. Lambdin, "Egypt: Its Language and Literature," and John A. Wilson, "Egyptian Culture and Religion," in *The Bible and the Ancient Near East: Essays in Honor of William Foxwell Albright,* ed. G. E. Wright (Garden City, N. Y., 1961).

[35] Erik Iversen, *The Myth of Egypt and Its Hieroglyphs in European Tradition* (Copenhagen, 1961).

[36] ". . . he was able to give an almost complete list of the hieroglyphical renderings of the names of the Egyptian rulers from Alexander to Antoninus Pius, and in September 1822 the result was submitted to the Académie des Inscriptions, and published in his famous 'Lettre à Mons. Dacier'. . . . The paper contained three plates with no less than 79 reproductions of royal names and their variants, together with a 'List of the phonetic signs in the hieroglyphic and demotic writings of the ancient Egyptians,' comprising twenty letters together with their 'homophones,' all of which were in all essentials correctly identified. The communication was deliberately confined to the establishment of these facts. . . . But towards the end of his article Champollion intimated a discovery of still greater importance and scope. He expressed the conviction that the phonetic signs were not only used in Hellenistic and Roman times for the exclusive writing of foreign names, and declared that he would soon be able to prove that they were original and integral elements of the hieroglyphical system as such. The actual demonstration and proof of this revolutionary assertion was reserved for a subsequent publication, but it was based on solid evidence, and a brilliant discovery, in fact, the true decipherment. . . . The understanding had come as a revelation . . . but he understood at once that the problem was solved, and the alphabetical nature of the hieroglyphs discovered. In a fever of excitement he was just able to rush to his brother's office at the nearby 'Institut de France,' where he threw his papers on his desk, pronounced his famous statement: 'Je tiens l'affaire' . . . and collapsed. He was present at the meeting at which his letter to Mons. Dacier was read, and had the satisfaction that de Sacy and most of the other members were at last convinced of the validity of his results, while Young, together with Jomard and an influential clique of disappointed decipherers and personal enemies refused to acknowledge their importance. For a while their voices were drowned in the clamour of success . . . and he was at long last able to concentrate on his next publication, the incomparable 'Précis du Système Hiéroglyphique,' which appeared in Paris in 1824. . . . His 'Grammaire Égyptienne,' a masterpiece of learning and erudition, was finished before 1830, but did not appear until 1836–41. . . . One would have thought that his improved conditions . . . would have made Champollion's last years peaceful and harmonious. Unfortunately this was not the case. For such is the nature of man that every scientific achievement of scope and importance must not merely pass through the unavoidable and indispensable judgement of honest criticism. It must also submit to the disgraceful ordeal and mock-trial of envy, ignorance and stupidity. The latter process is always painful, and not infrequently fatal to the victim, even if truth prevails, and a small but energetic clique of envious and malicious enemies and defamers succeeded in poisoning the last years of Champollion's existence. . . . His resistance, already impaired by overwork and the vicissitudes of his youth, failed, and he died in Paris on the 4th March 1832 at the age of 42. But Egyptology remains as a monument of his life-work." (Iversen, *Myth of Egypt,* 142–45.)

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In the latter part of the Twentieth Dynasty, the kings of the Ramesside line lost their power, and when the dynasty came to an end the country once again reverted to its ancient division into an upper and a lower half, each polarized around a leading city—Thebes and Tanis—which exercised control. Under the Twenty-second Dynasty, centralized royal power was restored, but subsequently lost again, until the Egyptianized Nubian kings ruling at Napata, in lower Nubia, invaded Egypt and once more set up a strong central government. Unluckily, this dynasty, the Twenty-fifth (ca. 730–663), fell victim to the Assyrian conquest of Egypt, which culminated in the sack of Thebes in 663. Assyrian rule, however, was brief, and from 650 to 525 Egypt was once more united under its own kings, ruling from the new capital city of Saïs. Saite rule, it is generally agreed, marks a period of revival: the nation once again became strong and prosperous, and the arts flourished. This is also the time in which Greek traders and soldiers flocked to Egypt, encouraged by the farsighted Saite kings to settle and make their contribution. The Persian conquest of 525 and the subsequent long Persian dominion were, no doubt, a political disaster. Yet when internal conditions had been stabilized, life went on much as before, Persian rule being the least oppressive of any foreign rule experienced anywhere in the ancient world. Even so, Egypt had the will and the strength to resume the fight for its independence. Persian dominion ended in 404, and under its native kings of the Twenty-eighth, Twenty-ninth, and Thirtieth Dynasties (404–341) Egypt made a spectacular recovery. Once again the arts flourished (the number and quality of monuments built in any period are the best indication of Egypt's strength and creative energy). Under Artaxerxes III, Persia rallied and reconquered its lost province (341), but this second period of Persian dominion was brief, ending with Alexander the Great's unopposed entry into Egypt in 332.

This then is the span of 750 years known as the Late Period, or Period of Decline (1080–332). In addition to the shrinking of political power and the repeated foreign invasions, symptoms of decline are seen in such phenomena as the imitation of Old Kingdom monuments by the craftsmen of the Saite age, the increasing rigidity of religious customs, the stereotyped formalism of official inscriptions, and the emergence of a caste system. I submit that the label "decline" has been fastened on this age prematurely and that the whole period has been so insufficiently studied that no generalization is justified at this time. A period of 750 years for which there exists only a single recent political history[37] is certainly not well studied. A few monographs have

[37] F. K. Kienitz, *Die politische Geschichte Ägyptens vom 7. bis zum 4. Jahrhundert vor der Zeitwende* (Berlin, 1953).

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dealt with certain aspects or phases,[38] and of course there are uncounted articles dealing with details. But the total volume of scholarly work done on the Late Period is only a fraction of that devoted to any of the earlier periods, although the source material is not especially scant.

In recent years, both the flowering and the decline of cultures have been the theme of several group discussions.[39] Relevant to the problem of cultural decline in ancient Egypt are the papers by Walther Wolf in *Aufstieg und Untergang* and W. H. McNeill in *Klassizismus und Kulturverfall.* Wolf distinguishes the following factors: a decline in Egypt's economic and political strength, which first became apparent during the Twentieth Dynasty in phenomena such as workmen's strikes, inflation, tomb robberies, and corruption in government. The causes for this decline he finds in the succession of fruitless wars; the loss of the Asiatic provinces; the replacement of bronze by iron, to which Egypt had no direct access; and excessive royal building. Closely entwined with this economic and political decline Wolf sees a cultural decline which he defines as a progressive exhaustion, an ebbing of vitality. He sees it manifested in social stagnation, that is, in appearance of a caste system, in an excessive use of magic running parallel with a rigid religious conservatism, and in imitative and archaizing art work—altogether a process of ossification ("Versteinerung") ending in a natural death. Wolf's analysis is the most thoughtful statement of the view that such a decline and death took place. In a tentative manner, McNeill questions this view and submits the theory that Egypt's culture in the first millennium B. C. was not in decline but in a state of successful stabilization, characterized by a high degree of homogeneity and integration. This challenge to the prevailing opinion is to be welcomed.

My own tentative view is this: In the latter part of the Twentieth Dynasty an economic and political decline did set in, but it was not an irreversible trend. It was a loss of strength comparable to that of the First and Second Intermediate Periods. It was followed by a thrice-repeated resurgence of strength, under the Twenty-fifth, Twenty-sixth, and Twenty-eighth–Thirtieth Dynasties. Ultimately Egypt fell to a power far greater than the Persians: Alexander and his successors. These seven centuries of political vicissitudes were accompanied by a cultural process of great complexity that scholars have

[38] E.g., Jürgen von Beckerath, *Tanis und Theben* (Glückstadt, 1951); Helene von Zeissl, *Äthiopen und Assyrer in Ägypten* (Glückstadt, 1955); Hermann Kees, *Das Priestertum im ägyptischen Staat vom Neuen Reich bis zur Spätzeit* (Leiden, 1953); Eberhard Otto, *Die biographischen Inschriften der ägyptischen Spätzeit* (Leiden, 1954); Georges Posener, *La première domination Perse en Égypte* (Cairo, 1936).

[39] *Aufstieg und Untergang der Grossreiche des Altertums,* ed. Mueller; *Klassizismus und Kulturverfall: Vorträge,* ed. G. E. von Grunebaum and Willy Hartner (Frankfurt a. M., 1960).

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barely begun to explore. Periods of artistic stagnation alternated with periods of great creativity. There were a weedy overgrowth of magic and, more sinister, a rigid religious fanaticism in place of the earlier tolerance. At the same time there were signs of striving and groping for new answers. The archaism of the Saite period has been greatly exaggerated. It was only sporadic and is no more a symptom of decline than Victorian Gothic. At least as significant as the archaizing tendencies are the attempts to assimilate Greek art. If anything, these attempts are proof of elasticity. But those who can see only decline dismiss in one breath the "decadence" of Saite art of the seventh, and of Greek influenced art of the fifth and fourth centuries, although the latter is truly experimental. In sum I believe that there were no cultural exhaustion and no natural death. Instead there were Alexander's conquest and thereafter the rule of a foreign, Greco-Macedonian elite that imposed its own culture. Then and only then, under the impact of Hellenism, did the native culture, no longer directed and supported from above, begin to disintegrate. Only then did it truly decline, but it did not die; it lingered on for another six hundred years, until it was extirpated, together with Hellenism, by the victorious Church. A culture is not an organism with a predetermined life span; it is the temporary crystallization of the ever-changing vision of man. Pharaonic culture did not flourish in a vacuum. It was a member of the family of ancient Near Eastern cultures that together had created the forward movement of mankind. It succumbed, together with its Near Eastern neighbors, to the successive impacts of Hellenism and Christianity. It came to an end when its particular world view was superseded by newer and wider visions.

The study of Egyptian history in Ptolemaic and Roman times is pursued mostly by classical scholars with only a few Egyptologists taking part. Since most of the source material is now written in Greek, this is natural enough, but it results in a somewhat unbalanced picture. Greco-Roman officialdom ruled, and Hellenistic and Roman culture dominated the land. Egypt's politics and culture were made in Alexandria, while the country at large became a backwater. Alexandria became the greatest city of the ancient world and the leading exponent of Hellenism. The Egyptians, meanwhile, had become "natives" and second-class citizens. They had, moreover, lost their voice. Pharaonic Egypt spoke with the double voice of king and commoner. But the Ptolemaic king was a foreigner who spoke for Hellenism, and the Egyptian, no longer having an official spokesman, fell silent. Literary production dried up. What remains are mostly nonliterary sources, that is, business documents such as sales, loans, tax receipts, and so forth. These add up to significant numbers

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and clarify economic matters. But, being written in demotic, they are not accessible to classical scholars, and they are, moreover, far outnumbered by documents written in Greek. Thus the countless studies that classical scholars have devoted to Greco-Roman Egypt, based as they are on Greek sources, reflect the viewpoint of the dominating Greek elite. Hence we do not have histories that trace the two nations and two cultures of Hellenistic Egypt and show their interrelation. In fact we do not even have an up-to-date political history of Greco-Roman Egypt, to replace the old works of E. R. Bevan and J. G. Milne.[40]

While ruthless Roman exploitation destroyed Egypt's wealth, and while the disastrous wars of the third century laid waste large parts of the splendid city of Alexandria, the Christian faith was reshaping Egyptian culture. At the end of the second century the Christian community of Egypt comes into view as an organized force, and from there on the sources multiply. The writings of the Church fathers, the legends of saints and martyrs, the "sayings" of the desert monks are all familiar sources. New is the great find of Coptic papyri, discovered by chance and furtively sold in 1945 or 1946, which after many years of tortuous negotiation eventually reached the safety of the Coptic Museum in Cairo. Coming from the vicinity of ancient Chenoboskion, this group of thirteen papyrus codices, containing forty-four separate works, represents the library of a Gnostic sect and is a find that in its context is of no lesser importance than that of the Dead Sea Scrolls. Since publication has barely begun, the full impact of this discovery will only be felt after many years. In any event they are the largest group of Gnostic works ever found and will immeasurably advance the study of the early Church's most dangerous rival.[41]

When the Gnostic wave had receded, Egypt was solidly Christian, and in the fourth century it made its specific contribution to Christianity: monasticism. The "Sayings of the Fathers"[42] portray with marvelous directness

[40] E. R. Bevan, *A History of Egypt under the Ptolemaic Dynasty* (London, 1927); J. G. Milne, *A History of Egypt under Roman Rule* (London, 1899; 3d ed., 1924). The best over-all account of Hellenistic Egypt is in the Egyptian chapters of Michael Rostovtzeff's masterly *The Social and Economic History of the Hellenistic World* (Oxford, Eng., 1941). Two brief syntheses, very useful within the limits of their brevity, are H. I. Bell's *Egypt from Alexander the Great to the Arab Conquest* (Oxford, Eng., 1948) and *Cults and Creeds in Graeco-Roman Egypt* (Liverpool, 1953; reprint, 1954, 1957). Wilhelm Schubart, *Ägypten von Alexander dem Grossen bis auf Mohammed* (Berlin, 1922), is a valuable and sensitive reconstruction of social and cultural life in town and countryside.

[41] For a preliminary survey of these papyri, see Jean Doresse, *The Secret Books of the Egyptian Gnostics* (New York, 1960). The original French edition, *Les livres secrets des gnostiques d'Égypte*, was published in Paris in 1958.

[42] *Apophthegmata patrum aegyptiorum;* cf. Marius Chaine, *Le manuscrit de la version copte en dialecte sahidique des apophthegmata patrum*, Institut français d'archéologie orientale, Bibliothèque d'études coptes, VI (Cairo, 1960). For a selection of these "Sayings" in exquisite English, see Helen Waddell, *The Desert Fathers* (Ann Arbor, Mich., 1957).

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the lives and thoughts of the anchorites of Nitria and Scete, while the cenobitic communities found their most powerful voice in the Sahidic writings of Shenute.[43] The most significant fact about the evolution of Christianity in Egypt is that it gathered the vital energies of the Egyptian nation which had found no outlet under Greco-Roman rule. In Coptic Christianity, Egypt once more became articulate and once more produced an indigenous culture. Though leadership at Alexandria was Hellenized, the Coptic Church became a truly national church. And as generations of Alexandrian bishops struggled to make their views prevail in the ecumenical councils, they could count on the solid support of monks, clergy, and laity. It is no accident that Shenute attended the Council of Ephesos and would have attended Chalcedon also, if death had not intervened. When Chalcedon had led to schism, Coptic Egypt stood behind its deposed patriarch, and, refusing all subsequent overtures from Byzantium, went its separate way. Thus, in the early centuries, the national character of the Coptic Church was a source of strength. But when it had resulted in the creation of a separatist, Monophysite Church, nationalism became a source of weakness. For, cut off from the mainstream of Christendom, and no longer nourished by the intellectual movements of Byzantium and Rome, the Coptic Church became stagnant. In due course, the Arab occupation of Egypt turned the Christian population into an ever-dwindling minority which reached its nadir in the eighteenth century.[44] The nineteenth century brought a gradual revival, a strengthening of the community, and reforms in the Church. In our own day, the Coptic community has created a number of learned institutions whose publications are making a major contribution to Coptic studies.[45] And thus these sons of ancient Egypt, though now a small minority in their own land, have attained the kind of consciousness that transforms the past, their own past, from a burdensome inheritance into the eternal life of the mind.

[43] On Shenute, see Johannes Leipoldt, *Schenute von Atripe und die Entstehung des national ägyptischen Christentums,* Texte und Untersuchungen zur altchristlichen Literatur, New Ser., 10/1 (Leipzig, 1903).

[44] A good history of Coptic Egypt is conspicuously lacking. Two brief sketches are W. H. Worrell, *A Short Account of the Copts* (Ann Arbor, Mich., 1945), and E. R. Hardy, *Christian Egypt* (New York, 1952). A recent French work, Sylvestre Chauleur, *Histoire des Coptes d'Égypte* (Paris, 1960), has gathered a good deal of material, but leaves much to be desired. Chiefly archaeological is Maria Cramer, *Das christlich-koptische Ägypten einst und heute* (Wiesbaden, 1959). A good monograph on the Coptic Church is Rudolf Strothmann, *Die koptische Kirche in der Neuzeit* (Tübingen, 1932).

[45] Notably the *Société d'Archéologie Copte,* founded in 1934, which publishes a *Bulletin* and several monographic series.

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The Teaching of African History

ROBERT I. ROTBERG *

AFRICAN history is a relatively recent addition to university curriculums. Now that it is taught as a subject in its own right, rather than as an aspect of European expansion, innumerable problems of organization and of syllabus selection have necessarily beset those who have ventured to give survey courses on the traditional European or American models. With the aid of few syntheses we have had to take account of a large body of new literature, both pioneering and revisionist in approach. Our knowledge of the African past has been augmented steadily since World War II; even so, the lacunae remain sufficiently numerous to bedevil attempts at strict conformity to the principles of continuity. Although inevitable in a new field of research, such gaps present problems of exposition. How, for example, are the early civilizations of Northeast Africa and the Nile Valley related to those of the East African littoral? What were the relations, in turn, between the Arab and Shirazi cities of East Africa, the Sudanic states of West Africa, and the great heart of Africa, from the Niger River to the Congo, Zambezi, and Limpopo Rivers? A number of scholars, generously assisted by foundations and governments, are today marrying different disciplines and approaches and providing tentative answers to these and other outstanding questions. Their writings, together with works long obscured or out of print, constitute the material around which a suitable syllabus must be fashioned.

There is as yet no standard approach to the teaching of African history. Based on my own somewhat limited experience, and on that of others actively engaged in similar research and teaching in the United States, Great Britain, and in the new universities of Africa, this proposed course outline is confined to tropical Africa and is designed for a standard academic year. Its organizational framework divides the year at about 1885, although either half could easily consume a full year itself. The impact and importance of the colonial experience justify devoting the larger portion of time to it. The bibliography for such a course should primarily mention only those titles

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which are reasonably available and suitable for students who read French and German with some difficulty.[1]

No completely satisfactory general history of tropical Africa exists. Roland Oliver and John D. Fage, *A Short History of Africa* (Harmondsworth, Eng., 1962), is the best introductory synthesis, but it is comparatively weak on the colonial period. Fage's *An Introduction to the History of West Africa* (Cambridge, Eng., 1962) is easily the best regional survey, however brief the compass.

Without the aid of a general text, we must construct a syllabus section by section. As a prologue, it is useful to acquaint students with the continent's prehistory and its geographical, social, and cultural setting. The geographical material is summarized in Walter Fitzgerald, *Africa: A Social, Economic and Political Geography of Its Major Regions* (London, 1955), in L. Dudley Stamp, *Africa: A Study of Tropical Development* (New York, 1953), and in the initial chapters of George Kimble, *Tropical Africa* (New York, 1960). The best of the regional treatments is R. J. Harrison Church, *West Africa* (London, 1960). Prehistory merits detailed study, and Henriette Alimen, *The Prehistory of Africa* (London, 1958), and L. S. B. Leakey, *Adam's Ancestors* (London, 1953), are useful books with which to begin. The best of the Penguin area series is J. Desmond Clark, *The Prehistory of Southern Africa* (Harmondsworth, Eng., 1959); it also includes Sonia Cole, *The Prehistory of East Africa* (Harmondsworth, Eng., 1954). *Cultures and Societies of Africa* (New York, 1960), edited by Simon and Phoebe Ottenberg, contains a well-chosen collection of social and cultural readings. *Continuity and Change in African Cultures* (Chicago, 1959), edited by William Bascom and Melville J. Herskovits, is more limited, but similarly useful.

A class might turn next to "Africa in the Ancient World." This subject includes discussion of the classical Mediterranean view of Africa, about 1000 B.C. to about A.D. 155, of the first age of African exploration, and of the structure of African society—as far as we are able to know it—during the same times. Lectures on Cush and Axum, the early civilizations of modern Sudan and Ethiopia, follow, beginning about 3000 B.C. and continuing until the fall of Axum in the eighth century. The classical sources are well known. Herodotus, Polybius, Pliny, Strabo, and Ptolemy, in their various translations, are all interesting. Perhaps the most accessible over-all treatment of the explorations is Max Otto Cary and Eric H. Warmington, *The Ancient Explorers*

[1] For a more complete bibliography, see my forthcoming "A History of Tropical Africa in Modern Times." In this article full titles of books are not given in all cases.

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(London, 1929), though the fullest commentary is in E. H. Bunbury's *A History of Ancient Geography* (2 vols., New York, 1959 [reprint of the 1883 ed.]). J. Oliver Thomson, *History of Ancient Geography* (Cambridge, Eng., 1948), contains detailed references. For a discussion of East Africa about the first century, the primary source is the first part of the anonymous *Periplus of the Erythraean Sea,* as translated by Wilfred Schoff (New York, 1912), pending the new Hakluyt Society version by Gervase Mathew. For Cush, students should consult the lucid discussion by A. J. Arkell, *A History of the Sudan from the Earliest Times to 1821* (London, 1961). Articles in the journals *Kush* and *Antiquity* are supplementary. Unhappily there is, in English, no such complete study of Axum. Chapters in J. Spencer Trimingham, *Islam in Ethiopia* (London, 1952), Edward Ullendorff, *The Ethiopians* (London, 1960), A. M. H. Jones and Elizabeth Monroe, *A History of Ethiopia* (Oxford, Eng., 1962), and Jean Doresse, *Ethiopia* (New York, 1959), must do. For a sixth-century account, *The Christian Topography of Cosmas,* translated by J. W. McCrindle (London, 1897), should be seen.

"Medieval Africa" encompasses East and West Africa in the seventh through the fifteenth century. Lectures on Ghana, Mali, Kanem-Bornu, Songhai, and the other civilizations and states of the Western Sudan may be followed by discussions of the coastal centers of East Africa before the Portuguese irruption, of East Africa's relations with China, India, and Arabia, of Monomotapa and the other cultures of Central Africa, and of Ethiopia from the fall of Axum to the end of the religious wars. For the Western Sudan, two books, both with defects, provide a good general coverage. Of the two, J. Spencer Trimingham, *A History of Islam in West Africa* (London, 1962), is more thoroughly based upon the Arabic, if not the traditional, sources. The other, E. W. Bovill, *The Golden Trade of the Moors* (London, 1961), is a more popular account, in many ways less useful than his earlier edition, published as *Caravans of the Old Sahara* (London, 1933). Among the more specialized studies, John D. Fage, *Ghana* (Madison, Wis., 1959), is excellent. His "Ancient Ghana: A Review of the Evidence" (*Transactions of the Historical Society of Ghana,* III [No. 2, 1957]) should be used in conjunction with Raymond Mauny, "The Question of Ghana" (*Africa,* XXIV [No. 3, 1954]). The best monographs on Mali, Songhai, and Kanem-Bornu are in French: Charles Monteil, "Les Empires du Mali" (*Bulletin du Comité d'Études Historiques et Scientifiques de l'Afrique Occidentale Française,* XII [Nos. 3–4, 1929]); Jean Rouch, *Contribution à l'histoire des Songhay* (Paris, 1953); Yves Urvoy, *Histoire de l'empire de Bornou* (Paris, 1949). Among the many important Arabic sources, only Ibn Battuta is readily accessible in a modern English

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translation by H. A. R. Gibb. The readings in *Nigerian Perspectives,* edited by Thomas Hodgkin (London, 1960), although limited geographically, provide a synopsis of the more significant primary material.

The scattered literature on medieval East Africa has recently been brought together in a standard work edited by Roland Oliver and Gervase Mathew, *History of East Africa* (3 vols., Oxford, Eng., 1963–), Volume I. G. S. P. Freeman-Grenville, who contributes two chapters to it, has another important discussion in "Swahili Literature and the History and Archaeology of the East African Coast" (*Journal of the East African Swahili Committee,* 28/2 [1958]). His new collection of contemporary accounts, *The East African Coast* (Oxford, Eng., 1962), is also excellent, and his thoughtful *The Medieval History of the Coast of Tanganyika* (New York, 1962) must be consulted. Other important articles may be found in *Tanganyika Notes and Records,* and in *Antiquity*. For relations with China, Jan Julius Duyvendak, *China's Discovery of Africa* (London, 1959), is definitive. Pending the publication of D. P. Abraham's thesis, his "The Early Political History of the Kingdom of Mwene Mutapa (850–1589)," *Historians in Tropical Africa* (Salisbury, Southern Rhodesia, 1962) is a seminal statement of an original view. H. A. Wieschoff, *The Zimbabwe-Monomotapa Culture in South-East Africa* (Menasha, Wis., 1941), is otherwise the most useful study.

The modern history of tropical Africa begins with the coming of the Portuguese. The narrative of their sea voyages is well known and may be dealt with briefly. For this purpose Edgar Prestage, *The Portuguese Pioneers* (London, 1933), serves admirably. The translated contemporary accounts are also of interest: Gomes Eannes de Azurara, *The Chronicle of the Discovery and Conquest of Guinea* (2 vols., London, 1896); *The Voyages of Cadamosto,* translated and edited by G. R. Crone (London, 1937); *Vasco Da Gama's First Voyage,* translated by Ernst Georg Ravenstein (London, 1898); and *The Voyage of Pedro Álvares Cabral to Brazil and India,* translated by William Brooks Greenlee (London, 1938). Other topics can include "West Africa," "The Congo and Angola," "South and East Africa," and "Ethiopia during Portuguese Times." For the first, John William Blake, *European Beginnings in West Africa* (London, 1937), and his collection of contemporary documents, *Europeans in West Africa, 1450–1560* (2 vols., London, 1942), are basic. Also of importance is Duarte Pachecho Pereira, *Esmeraldo de Situ Orbis,* translated by George Kimble (London, 1937). The Congo, Angola, and Mozambique are discussed in James Duffy, *Portuguese Africa* (Cambridge, Mass., 1959). Charles R. Boxer, *Salvador de Sá and the Struggle for*

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Brazil and Angola (London, 1952), and the last part of Gladwyn Murray Childs, *Umbundu Kinship and Character* (London, 1949), are also relevant.

For Southern and Eastern Africa, Eric Axelson, *South-East Africa, 1488–1530* (London, 1940), and his somewhat undigested *The Portuguese in South-East Africa 1600–1700* (Johannesburg, 1960) are necessary. The Oliver and Mathew volume, cited above, is again important. Justus Strandes, *Die Portugiesenzeit von Deutsch- und Englisch-Ostafrika* (Berlin, 1899), of which there is an abridged translation entitled *The Portuguese Period in East Africa* (Nairobi, 1961), is dated but still useful. A good account is in Charles R. Boxer and Carlos de Azevedo, *Fort Jesus and the Portuguese in Mombasa* (London, 1960). For Prester John and the Portuguese in Ethiopia, one may use E. Denison Ross, "Prester John and the Portuguese in Ethiopia," in *Travel and Travellers of the Middle Ages,* edited by Arthur Percival Newton (London, 1926), and *The Prester John of the Indies,* edited by C. F. Beckingham and G. W. B. Huntingford (2 vols., Cambridge, Eng., 1961), and Jones and Munroe, cited above.

The interaction of Africa, Europe, and America occasioned by the slave trade merits a short series of lectures tracing the development of European interest after the first Portuguese voyages, the subsequent competition among European nations for slave cargoes resulting, as it did, in the rise of Europe on the coasts of Western Africa, and, in human terms, the nature and extent of the commerce itself. Unfortunately, there is no general history on the slave trade as a whole. Basil Davidson, *Black Mother* (Boston, 1961), is a usable and popular introduction. But for a full appreciation one must consult the massive *Documents Illustrative of the History of the Slave Trade to America,* edited by Elizabeth Donnan (4 vols., Washington, D. C., 1930–35); André Ducasse, *Les Négriers, ou Le trafic des esclaves* (Paris, 1948); and, for a more modern period, Colin W. Newbury, *The Western Slave Coast and Its Rulers* (Oxford, Eng., 1961). The reprehensible aspects of the trade may be illustrated by extracts contained in *Slave Ships and Slaving,* edited by George Francis Dow (Salem, Mass., 1927), some of which are in Donnan's books, and by the accounts in Edmund D'Auvergne, *Human Livestock* (London, 1933). For the European involvement, see K. G. Davies, *The Royal African Company* (London, 1957); Boxer, *Salvador de Sá;* Duffy, *Portuguese Africa;* the relevant portions of W. Walton Claridge, *A History of the Gold Coast and Ashanti* (2 vols., London, 1915), Volume I; and John Milner Gray, *A History of the Gambia* (Cambridge, Eng., 1940). Of the abundant contemporary narratives the most readable are Thomas Phillips, *A Journal of a Voyage . . . from England to . . . Africa . . . and So Forward to Barbadoes,* in Awnsham and John

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Churchill, *A Collection of Voyages and Travels* (7 vols., London, 1732), Volume VI; William Bosman, *A New and Accurate Description of the Coast of Guinea* (London, 1721); James Barbot, "A Voyage to New Calabar," in Churchill, *Voyages and Travels,* Volume V; Alexander Falconbridge, *An Account of the Slave Trade on the Coast of Africa* (London, 1788); William Snelgrave, *A New Account of Some Parts of Guinea and the Slave Trade* (London, 1734); and *Efik Traders of Old Calabar,* edited by Daryll Forde (London, 1956).

Chronologically, the lecturer may at this juncture retrace his centuries to return to the African kingdoms and city-states that were dealt with initially under the rubric "Medieval Africa." I call this section "The Rise and Fall of New States, 1492–1885." The first meetings touch upon later Songhai, Bornu, and the Hausa communities. The same sources—Trimingham, Bovill, Rouch, and Urvoy—are again relevant. John Pory's translation of Leo Africanus, *The History and Description of Africa* (3 vols., London, 1896), makes very absorbing reading. Hodgkin once again contains other apposite contemporary accounts. Subsequent lectures carry the Western and Central Sudan to the time of partition: we deal with the Fulani conquest of Hausaland, the modern history of Bornu, and the nineteenth-century states created by the conquerors Hamadu of Maçina, al-Hajj 'Umar, and Samori. In addition to Trimingham and Hodgkin, the rather dated accounts in Flora Shaw, *A Tropical Dependency* (London, 1905), and S. J. Hogben, *Muhammadan Emirates in Nigeria* (London, 1930), there is a good summary of the Fulani wars in Michael Crowder, *A Short History of Nigeria* (New York, 1962). There is also a stimulating reconstruction in Michael G. Smith, *Government in Zazzau, 1800–1950* (London, 1959).

In the modern period, from 1492 to 1855, we can begin to talk knowledgeably about the states of the West African forest and savanna. The Yoruba-speaking kingdoms of Ife and Oyo, Benin, Warri, Nupe, Mossi, Dahomey, Akwamu, and Ashanti each deserve examination. For the Nigerian states, Crowder again has a summary, and Hodgkin contains pertinent contemporary writings. But we must also use the somewhat circumstantial traditional renderings in Samuel Johnson, *The Yorubas* (Lagos, Nigeria, 1956), and Jacob Egharevba, *A Short History of Benin* (Ibadan, Nigeria, 1960). On Ife, see Frank Willett, "Ife and Its Archaeology" (*Journal of African History,* I [No. 2, 1960]); on Dahomey, see Newbury, cited above, and Archibald Dalzel, *The History of Dahomey* (London, 1793); on Benin, see Blake, above, and the summary in R. E. Bradbury, *The Benin Kingdom* (London, 1957); on Mossi, see Lucien Marc, *Le Pays Mossi* (Paris, 1909); and on Akwamu, see Ivor

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Wilks, "The Rise of the Akwamu Empire 1650–1710" (*Transactions of the Historical Society of Ghana,* III [No. 2, 1957]). The history of Ashanti is contained in more accessible sources—William E. F. Ward, *A History of Ghana* (London, 1958), and Fage, *Ghana.* For a complete treatment of the Ashanti-British involvement, however, Claridge, above, must be used in conjunction with Margaret Priestley, "The Ashanti Question and the British" (*Journal of African History,* II [No. 1, 1961]), and Margaret Priestley and Ivor Wilks, "The Ashanti Kings in the Eighteenth Century: A Revised Chronology" (*Journal of African History,* I [No. 1, 1960]).

The destinies of Europe and Africa became more intimately intertwined in the eighteenth and nineteenth centuries. This section leads up to partition and includes numerous strands, many of which can only be dealt with in a shortened compass. A number of lectures can trace the extension of European interest inland from both coasts as a result of new economic and strategic compulsions and the initial indigenous response to these new incursions. The first British colony, Senegambia, deserves some attention, and the founding of Sierra Leone and Liberia follow. Before it was possible for Europe to obtain a firm hold on the continent's interior, the third age of European exploration was necessary. One begins logically with the Association for Promoting the Discovery of the Interior Parts of Africa, Mungo Park, the Niger quest, David Livingstone and the heart of Africa, and the later explorations of East Africa. The third part of this section may be termed "Tentative Partition: The New Colonies," with a discussion of the Gold Coast and Ashanti, the Niger coast, France from Senegambia to Loango, and Britain and France in the Indian Ocean and on the coast of East Africa.

When we come to eighteenth- and early nineteenth-century East Africa, we may once again use Oliver and Mathew, and, for British policy, Reginald Coupland's *East Africa and Its Invaders* (Oxford, Eng., 1938) and *The Exploitation of East Africa* (London, 1939) are full. John Milner Gray, *The British in Mombasa, 1824–1826* (London, 1957), is thorough, and *General Rigby, Zanzibar, and the Slave Trade,* edited by Mrs. Charles E. B. Russell (London, 1935), contains contemporary letters and diaries. Unhappily, in the English language there is no adequate presentation of the French involvement. Herbert Ingram Priestley's two studies, *France Overseas through the Old Regime* (Berkeley, Calif., 1939) and *France Overseas: A Study of Modern Imperialism* (New York, 1938), can be used. Stephen H. Roberts, *History of French Colonial Policy, 1870–1925* (2 vols., London, 1929), is more valuable for a later period. A limited French summary is contained in Maurice Delafosse, "Afrique Occidentale Française," in the *Histoire des colonies françaises,* edited by Gabriel

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Hanotaux and Alfred Martineau (Paris, 1931). A more usable account is Jules François Saintoyant, *La Colonisation française sous l'ancien régime* (2 vols., Paris, 1929). André Arcin, *Histoire de la Guinée française* (Paris, 1911), and Prosper Cultru, *Les Origines de l'Afrique occidentale: Histoire du Sénégal du* XV^e^ *siècle à 1870* (Paris, 1910), are important. From the British viewpoint, Senegambia is treated in Gray, *A History of the Gambia,* and in Eveline C. Martin, *The British West African Settlements, 1750–1821* (London, 1927). Christopher Fyfe's monumental *A History of Sierra Leone* (London, 1962) is an exhaustive treatment of that colony in the eighteenth and nineteenth centuries only, but his *A Short History of Sierra Leone* (London, 1962), which also discusses the modern period, will be read more easily. There is no such thorough study of Liberia. Charles Henry Huberich's *The Political and Legislative History of Liberia* (2 vols., New York, 1947) is detailed and diffuse. For the American side, P. J. Staudenraus, *The African Colonization Movement 1816–1865* (New York, 1961), is authoritative, but both books must be supplemented with articles by Henry Noble Sherwood and others in the *Journal of Negro History*. Nnamdi Azikiwe, *Liberia in World Politics* (London, 1934), is also useful.

No one has yet written a history of the third age of African exploration. There are a number of anthologies, of which the most useful are *West African Explorers,* edited by Cecil Howard (London, 1955); Margery Perham and Jack Simmons, *African Discovery* (London, 1957); and Charles Richards and James Place, *East African Explorers* (London, 1960). Of the biographies of Livingstone, William Garden Blaikie's *The Personal Life of David Livingstone* (New York, 1881) and George Seaver's *David Livingstone: His Life and Letters* (London, 1957) are standard, although Simmons' shorter study, *Livingstone and Africa* (London, 1955), is excellent. Among the many books written by the explorers themselves, I would recommend Dixon Denham *et al., Travels and Discoveries in Northern and Central Africa* (4 vols., London, 1813), and *Travels of Mungo Park,* edited by Ronald Miller (London, 1954); Heinrich Barth, *Travels and Discoveries in North and Central Africa* (5 vols., London, 1857); David Livingstone, *Missionary Travels and Researches in South Africa* (2 vols., London, 1857); James Bruce, *Travels to Discover the Source of the Nile* (5 vols., Edinburgh, 1805); Richard Francis Burton, *The Lake Regions of Central Africa* (New York, 1961); and John Hanning Speke, *Journal of the Discovery of the Source of the Nile* (New York, 1868).

When we move to "Tentative Partition," particularly in West Africa, John D. Hargreaves, *Prelude to the Partition of West Africa* (London, 1963),

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and Ward and Fage, cited above, are the best introductions, although Claridge, again, is complete. George Maclean's proconsulship is the subject of G. E. Metcalfe's recent *Maclean of the Gold Coast* (London, 1962). Joseph B. Danquah, "The Historical Significance of the Bond of 1844" (*Transactions of the Historical Society of Ghana,* III [No. 1, 1957]), is important. For the Niger coast, see Newbury, cited above, K. Onwuka Dike, *Trade and Politics in the Niger Delta, 1830–1885* (Oxford, Eng., 1956), and Saburi O. Biobaku, *The Egba and Their Neighbours, 1842–1872* (Oxford, Eng., 1957). Crowder, above, and Alan F. Burns, *History of Nigeria* (London, 1955), discuss aspects of Nigerian history prior to partition.

The scramble for and the partition of tropical Africa were followed by widespread colonial rule, by the Westernization of the African interior, and, in our own time, by the emergence of a number of modern states. The importance of partition cannot be overestimated; the second semester might, therefore, begin with an extended series of lectures on the various European diplomatic and military conquests of the "Dark Continent." The West African Conference at Berlin (1884–1885) illustrates the diplomatic and strategic considerations that precipitated the scramble and is an important prelude in its own right. As a result of the conference and of King Leopold II's careful maneuvers, the powers of Europe and America gave the Congo Independent State international standing. Leopold and his ministers capitalized on it and constructed a vast colonial empire, which was a subject of continual controversy until its transfer to the Belgian state in 1908. Two or three sessions could easily be devoted to its controversial life. Bismarck's Germany was another new colonial power, and its acquisitions in Africa (1884–1914) merit detailed exposition. Britain and France were the most involved partitioners, however, and their actions and rivalry call for lengthy analysis. Among the more important topics worthy of discussion are: "George Goldie and Gaining of Commercial Hegemony on the Niger," "Lord Lugard and the Conquest of the Northern Emirates," "The Humbling of Ashanti," "The Struggle with Germany for East Africa" (which resulted in the "protection" of Buganda and the arbitrary division of what later became Kenya and Tanganyika), "The Conquest of the Rhodesias," "The French Drive to the Upper Niger," and "The French Exploitation of Equatoria." In general, these topics cover the period 1885–1905, to be followed by a discussion of the twentieth-century colonies, area by area.

Surprisingly, there is no good over-all study of the partition based upon African materials and considerations. For the diplomatic background, Wil-

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liam L. Langer's two classic studies, *European Alliances and Alignments, 1871–90* (New York, 1950) and *The Diplomacy of Imperialism, 1890–1902* (New York, 1951), are still essential. For a stimulating consideration of British policy, see Ronald Robinson and John Gallagher, *Africa and the Victorians: The Climax of Imperialism in the Dark Continent* (New York, 1961). Sybil Eyre Crowe, *The Berlin West African Conference, 1884–1885* (London, 1942), remains the only solid treatment of those negotiations. Arthur Berriedale Keith, *The Belgian Congo and the Berlin Act* (Oxford, Eng., 1919), is a useful supplement. A recommended accessible study of the Congo Independent State, although based largely upon secondary sources, is Ruth Slade, *King Leopold's Congo* (London, 1962).

There is no good general English discussion of German policies in Africa. On the Cameroons, however, Harry Rudin, *Germans in the Cameroons, 1884–1914* (London, 1938), remains standard. Heinrich Vedder, *South West Africa in Early Times* (London, 1938), a translation, is usable but limited, as is William Osgood Aydelotte, "The First German Colony and Its Diplomatic Consequences" (*Cambridge Historical Journal,* V [No. 3, 1937]). A. J. P. Taylor, *Germany's First Bid for Colonies* (London, 1938), and Mary Evelyn Townsend, *The Rise and Fall of the German Colonial Empire* (New York, 1930, are Europe centered. The history of early German and British East Africa may conveniently be read in Kenneth Ingham, *A History of East Africa* (London, 1962). For Uganda, however, his version must be used in conjunction with Margery Perham, *Lugard: The Years of Adventure* (London, 1956), and articles by Roland Oliver, John Milner Gray, and others in the *Uganda Journal.*

For the Niger quest, Crowder's work is preferable to that of Burns. In addition, Arthur Norton Cook, *British Enterprise in Nigeria* (Philadelphia, 1943), and William Nevill Montgomerie Geary, *Nigeria under British Rule* (London, 1927), are good. But the most readable accounts appear in John Flint, *Sir George Goldie and the Making of Nigeria* (London, 1960), and in Perham's volume. Newbury, *The Western Slave Coast and Its Rulers,* is once again important for Nigeria as well as for Dahomey and Togo. The Gold Coast-Ashanti involvement and the latter's subsequent denouement are easily understood in Ward, cited above (a good summary of Claridge). Philip Mason, *The Birth of a Dilemma* (London, 1958), and Alexander John Hanna, *The Beginnings of Nyasaland and North-Eastern Rhodesia, 1859–95* (Oxford, Eng., 1956), cover Central Africa.

The French campaigns in West Africa are conveniently detailed in Delafosse, cited above, in Paul Gaffarel, *Notre Expansion Coloniale en Afrique*

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(Paris, 1918), and in a number of country histories by French writers. Unhappily, Priestley and Roberts, both cited above, are the only English sources, and both are cursory. Auguste Terrier, "Afrique Equatoriale Française," in the Hanotaux and Martineau volume, is complementary to the Delafosse contribution. Otherwise, we must fall back upon Fernand Rouget, *L'Expansion Coloniale au Congo Français* (Paris, 1906).

Portugal, already established in Africa, played a secondary role in the scramble. That country filled out its colonial hinterland, lost the Rhodesias and Nyasaland to Great Britain, and proceeded to rule Angola and Mozambique in a tradition that it had pioneered. Three main topics emerge: the reawakening of Portuguese interest in its colonies during the early part of the nineteenth century; the conquest of the Angolan and Mozambique interiors as a by-product of partition; and modern colonial rule under Antonio Salazar. A knowledge of Portuguese is required for a thorough understanding of this part of the course, but Duffy, *Portuguese Africa,* provides an accessible introduction. The travel narratives of Hermenigildo Capello and Roberto Ivens, *From Benguella to the Territory of Yacca* (London, 1882), and Serpa Pinto, *How I Crossed Africa* (2 vols., Philadelphia, 1881), may be read in the English translations. Joachim John Monteiro, *Angola and the River Congo* (2 vols., London, 1875), contains important descriptions of Portuguese colonial conditions before the time of partition. For this earlier period, Childs, *Umbundu Kinship and Character,* is again supplementary. Philip R. Warhurst, *Anglo-Portuguese Relations in South-Central Africa, 1890–1900* (London, 1962), contains a discussion of the more important imbroglios in the Mozambique region. Charles Nowell, "Portugal and the Partition of Africa" (*Journal of Modern History,* XIX [No. 1, 1947]), is also significant, though Europe centered. In examining the problem of forced labor and slavery in the Portuguese colonies, Henry Nevinson, *A Modern Slavery* (New York, 1906), William Cadbury, *Labour in Portuguese West Africa* (London, 1910), and Basil Davidson, *The African Awakening* (London, 1955), are descriptive. F. Clement C. Egerton, *Angola in Perspective* (London, 1957), is an apologia. Some of the recent periodical literature on Portugal in Africa is of interest, although Lucy Mair, *Native Policies in Africa* (London, 1936), still provides the most convenient summary of President Salazar's early colonial policies and conceptions.

French-speaking Africa from about 1905 to 1960 forms another unit. Within it, West Africa, Equatorial Africa, and the Congo may be treated separately. In the first geographical region three themes—the imposition of direct rule, the development of the economy, and the emergence of national-

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ism—stand out. In Equatorial Africa, the French spent the better part of the first two decades of this century occupying the northern half of the territory and in consolidating their rule. At the same time they continued a pattern of harsh exploitation that had been a feature of the period of partition. There, too, the administrative pattern of direct rule and "assimilation" provided the background to nationalism.

From 1908 to 1960 Belgium, rather than its king, governed the Congo. Its colonial rule was heralded by important liberalizing reforms and by a new rule of law. In other ways, however, the Congo remained a source of wealth for European and American investors. Discussions of the administrative bases, the economic achievement, and the regimentation of the indigenous African population may be followed by an examination of the African reaction, from Simon Kimbangu, a religious nationalist of the 1920's, to Patrice Lumumba, the first leader of an independent Congo.

In English, Raymond Leslie Buell, *The Native Problem in Africa* (2 vols., New York, 1928), Mair's work, Roberts, *History of French Colonial Policy,* and Kenneth Robinson's Senegal chapter, in W. J. M. Mackenzie and Kenneth Robinson, *Five Elections in Africa* (Oxford, Eng., 1960), are the most useful for French policies, although none is comprehensive. Of the more popular approaches by Virginia Thompson and Richard Adloff, *French West Africa* (Stanford, Calif., 1958), and *The Emerging States of French Equatorial Africa* (Stanford, Calif., 1960), the second is the more perceptive. The first confines itself, for the most part, to the period 1946–1957. In French, André Villard, *Histoire du Sénégal* (Dakar, 1943), and F. J. Amon d'Aby, *La Côte d'Ivoire dans la cité africaine* (Paris, 1951), are good for the respective regions. Among the most important studies of the French Congo is Georges Balandier, *Sociologie actuelle de l'Afrique Noire* (Paris, 1955).

Unhappily, when one crosses geographically to what was the Belgian Congo, one finds a complete absence of good studies in English. Buell and Mair are again valuable for the early period. Thereafter one turns to George van der Kerken, *La politique coloniale belge* (Antwerp, 1943), to Colin Legum's brief *Congo Disaster* (Harmondsworth, Eng., 1961), to the patriotic statement of Belgian intention in Pierre Ryckmans, "Belgian 'Colonialism'" (*Foreign Affairs,* XXXIV [No. 1, 1955]), to Jean Labrique, *Congo politique* (Leopoldville, 1957), and to an article by one intimately connected with the recent emergence of Congo nationalism—A. A. J. Van Bilsen, "Some Aspects of the Congo Problem" (*International Affairs* [London], XXXVIII [No. 1, 1962]).

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"English-speaking West Africa," another collective, embraces the five particular examples of Nigeria, Ghana, Sierra Leone, the Gambia, and Liberia. In this region the administrative, social, and economic policies of Great Britain as a colonial power were implemented in divergent local ways. In the Gold Coast, the conquest of Ashanti and the northern territories was accompanied by an intensification of control within the traditional sphere of British coastal influence. After World War I, successive governors attempted to integrate that region with the more recently acquired hinterland and to encourage social, economic (cocoa was basic), and political development. Slow evolution was succeeded, after World War II, by the intensification of a struggle by Ghanaians to free themselves from foreign domination, and by an independence in 1957 that proved an object worthy of emulation elsewhere in Africa. David Kimble, *A Political History of Ghana: The Rise of Gold Coast Nationalism, 1850–1928* (Oxford, Eng., 1963), is definitive. F. M. Bourret, *Ghana: The Road to Independence, 1919–1957* (London, 1960), is a good study that concentrates on the 1919–1927 period. Ward, *History of Ghana,* is again useful, and there is a section in Buell. Martin Wight, *The Gold Coast Legislative Council* (London, 1947), is informative. For more recent decades, George Padmore, *The Gold Coast Revolution* (London, 1953), provides an overview; David Apter, *The Gold Coast in Transition* (Princeton, N. J., 1955), is thoughtful, but overly methodological; and Dennis Austin, "The Working Committee of the United Gold Coast Convention" (*Journal of African History,* II [No. 2, 1961]) is, though restricted in scope, an extremely able and thorough analysis of a crucial period in modern African politics. President Kwame Nkrumah's autobiographies, *Ghana* (Edinburgh, 1957), and *I Speak of Freedom* (New York, 1961), are also worth assigning.

Nigeria in its modern form was to a large extent created by Lord Lugard. He sought to tie north to south, politically and economically. He also introduced an *ad hoc* arrangement that became the model form of "indirect rule." Successive governors elaborated the nature and form of this administrative arrangement and diffused it to other British possessions. The emergence of Nigeria under alien rule was a complicated combination of educational and social advances which, in time, were subsumed under the heading "nationalism." The reaction to colonial government may be traced, as in Ghana, from nineteenth-century catalysts to the formation of African associations and parties. Once Ghana was on its way, independence for Nigeria came with relative ease. Crowder's *Short History of Nigeria* is, as before, the best summary. For the earlier period, Margery Perham, *Lugard: The Years of Authority* (London, 1960), is essential. Cook and Geary again contain ma-

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terial of interest. Joan Wheare, *The Nigerian Legislative Council* (London, 1950), traces the development of an important political institution in the manner of Wight. Margery Perham's reissued *Native Administration in Nigeria* (London, 1962) is classic. Also basic to any understanding of the modern period is James S. Coleman, *Nigeria: Background to Nationalism* (Berkeley, Calif., 1960). The autobiography of Obafemi Awolowo, *Awo* (Cambridge, Eng., 1960), is very provocative.

Sierra Leone may be discussed more succinctly. Following a century of control over the tiny colony, Britain, after 1896, overcame indigenous opposition, acquired its hinterland, and thereby gave the state of Sierra Leone its contemporary configuration. Economic and political changes were to some extent parallel to Ghana and Nigeria, although the tension between Creole and African has been a continuous feature particular to itself. In time indigenous pressure, as elsewhere, persuaded Britain to transfer the machinery of government to local personnel and parties. Fyfe's *Short History* is best. J. D. Hargreaves, "The Establishment of the Sierra Leone Protectorate and the Insurrection of 1898" (*Cambridge Historical Journal,* XII [No. 1, 1956]), explains the imposition of British rule in the hinterland. Pending a monograph by Martin Kilson, a discussion of modern politics can best be found in his "Social Forces in West African Political Development" (*Journal of Human Relations,* VIII [Nos. 3–4, 1960]), and in D. J. M. Scott, "The Sierra Leone Elections, May 1957," in the Mackenzie and Robinson volume previously mentioned.

Liberia provides an interesting special case that has been too little studied. Two periods naturally recommend themselves: from independence (1847) to the rise of active United States intervention (1912), and from then to the present. The development of local politics, a perverse copy of nineteenth-century America, was assisted by the introduction of a foreign-controlled economy. The subjugation of indigenous Africans by Americo-Liberians is a further theme. The 1847–1912 era is discussed, however inadequately, in Harry Johnston, *Liberia* (London, 1906), and Thomas H. B. Walker, *History of Liberia* (Boston, 1921). Buell and Azikiwe, both cited above, deal with the early part of the second period. The best modern treatment is J. Gus Liebenow, "Liberia," in *African One-Party States,* edited by Gwendolen M. Carter (Ithaca, N. Y., 1962). Buell's brief *Liberia: A Century of Survival 1847–1947* (Philadelphia, 1947) is a scathing overview. Also worth noting are Raymond W. Bixler, *The Foreign Policy of the United States in Liberia* (New York, 1957); William E. Burghardt Du Bois, "Liberia, the League and the United States," in *Africa Seen by American Negroes* (Paris, 1958); and

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Nathaniel R. Richardson, *Liberia's Past and Present* (London, 1959).

White settlers have fought the indigenous inhabitants of British Central Africa—the Rhodesias and Nyasaland—since the time of partition. As the colonial frontier moved north from the Transvaal and Bechuanaland, so whites came to the Rhodesias and, latterly, to Nyasaland, in order to make permanent homes for themselves. After the successful suppression of African discontent, Southern Rhodesians quickly achieved *de facto* home rule and, in 1923, true self-government. To a local white government, Africans were of secondary importance, and action meant to assist them, to administer them, and to regiment them provides the necessary background to any discussion of nationalism. Both Northern Rhodesia and Nyasaland remained protectorates administered on a fairly standard British pattern. Northern Rhodesia became the world's second largest producer of copper, however, and its political and economic development became almost completely geared to mineral extraction. All three territories, long sharing a community of interest, were joined in 1953 as the Federation of Rhodesia and Nyasaland, the creation of which merits detailed discussion because of its great political impact. Africans reacted vigorously, intensifying their agitation for freedom from alien control. Nyasaland, with the smallest number of whites and the most dynamic African leader, was the first to achieve success after violence had signified the extent of indigenous discontent.

The only general, albeit severely limited, introduction to modern British Central Africa is Alexander John Hanna, *The Story of Rhodesia and Nyasaland* (London, 1960). The abortive, but significant, Nyasaland rebellion of 1915 is treated definitively in George Shepperson and Thomas Price, *Independent African* (Edinburgh, 1958). A readable, sensible study of early Southern Rhodesia is to be found in Mason's previously mentioned volume. Colin Leys, *European Politics in Southern Rhodesia* (Oxford, Eng., 1958), is essential for an appreciation of the more recent period. Lewis Gann, *The Birth of a Plural Society* (Manchester, Eng., 1958), is a brief, useful study of early Northern Rhodesia. For mid-twentieth-century Nyasaland, Northern Rhodesia, and Southern Rhodesia, sections of Richard Gray's excellent *The Two Nations* (London, 1960) must suffice. Clyde Sanger, *Central African Emergency* (London, 1960), gives a good journalist's view of events leading up to the crisis of 1958–1959.

The recent history of British East Africa contains five major themes: the internal events of Kenya, Tanganyika, and Uganda; the quest for closer union; and the assumption of African independence. In Kenya, settlers early asserted a predominance more on the Central African model. Their own

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struggle for self-government is interesting if taken together with the development of discriminatory policies toward Africans and Asians. The government of Tanganyika, with mandatory responsibilities, never proved as amenable to settler pressure. The introduction of indirect rule there was only one indication of a somewhat different approach. In Uganda the development of British institutions and economic practices proceeded against a background of local Ganda nationalism. Over-all, settlers and some British governments wanted closer union, which Africans opposed. The development of nationalism, focused in Kenya on competition for land, grew in all three territories, although in Uganda its natural development was hindered by the peculiar ties of Buganda.

Ingham, *History of East Africa,* is a general introduction. The settler factor in Kenya history is faithfully portrayed in Elspeth Huxley's biography of Lord Delamere, *White Man's Country* (2 vols., London, 1953), and in Marjorie Ruth Dilley, *British Policy in Kenya Colony* (New York, 1937). W. McGregor Ross, *Kenya from Within* (London, 1927), contains a full account of local politics from a viewpoint sympathetic to African and Asian grievances. The best study of Tanganyika under the British regime is an unpublished thesis by Margaret L. Bates. Pending its appearance in published form, J. Clagett Taylor, *The Political Development of Tanganyika* (Stanford, Calif., 1963), Bernard T. G. Chidzero, *Tanganyika and International Trusteeship* (London, 1961), sections of Donald Cameron, *My Tanganyika Service and Some Nigeria* (London, 1939), and parts of Buell, above, must suffice. Uganda is more amply covered in the modern period. D. Anthony Low and R. Cranford Pratt, *Buganda and British Overrule* (London, 1960), and David Apter, *The Political Kingdom in Uganda* (Princeton, N. J., 1961), are good, but less than full. Kenneth Ingham, *The Making of Modern Uganda* (London, 1958), and articles in the *Uganda Journal* are supplementary. The recent history of Zanzibar, unhappily, has not received the attention of many scholars, but L. W. Hollingsworth, *Zanzibar under the Foreign Office* (London, 1953), is a beginning. Jomo Kenyatta's reissued *Facing Mount Kenya* (London, 1961) is instructive when African reactions are discussed. A number of articles focus on phases of nationalism. Among the more useful are R. Cranford Pratt, "Nationalism in Uganda" (*Political Studies,* IX [No. 2, 1961]), Martin Kilson, "Land and Politics in Kenya" (*Western Political Quarterly,* X [1957]), George Bennett, "The Development of Political Organizations in Kenya" (*Political Studies,* V [No. 2, 1957]). D. Anthony Low, *Political Parties in Uganda, 1949–62* (London, 1962), is also of value.

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A last section, on Ethiopia, Somalia, and the Sudan, should account for foreign intervention in all three, periods of alien rule and indigenous reaction, and a reassertion of the sovereignty of the one, together with the achievement of independence of the other two. In Ethiopia an early phase and a late phase of European interest were followed by invasion. There had been, meanwhile, internal struggles for the throne, out of which Haile Selassie emerged victorious. To the south and east, Britain and Italy assumed control of Somaliland. In the Sudan the aftermath of the defeat of the Mahdi brought about an Anglo-Egyptian condominium and the energetic transformation of the Sudan into a country dependent economically upon the cultivation of irrigated cotton. Source material in English is varied. For Ethiopia, Margery Perham, *The Government of Ethiopia* (London, 1948), and David James Mathew, *Ethiopia: The Study of a Polity, 1540–1935* (London, 1947), are recommended. The Sudan is adequately served by P. M. Holt, *A Modern History of the Sudan* (London, 1961), although L. A. Fabunmi, *The Sudan in Anglo-Egyptian Relations, 1800–1956* (New York, 1960), and K. D. D. Henderson, *The Making of the Modern Sudan* (London, 1953), are also useful. On Somalia there is Enrico Cerulli, *Somalia* (2 vols., Rome, 1959), in Italian. E. E. Drake-Brockman, *British Somaliland* (London, 1912), and Douglas Jardine, *The Mad Mullah of Somaliland* (London, 1953), are informative, if hardly painstaking. G. K. N. Trevaskis, *Eritrea: A Colony in Transition* (London, 1960), is a discussion of the most recent phase of that region of Ethiopia.

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* * * *Notes and Suggestions* * * *

Time to Reclaim: The Current Challenge of American Constitutional History

PAUL L. MURPHY*

ONE aspect of the public furor over the Supreme Court's decision striking down the New York Regent's prescribed public school prayer[1] should both bother the consciences and arouse the energies of historians. This was the frequent complaint that Justice Hugo Black in his majority opinion placed his reliance almost solely upon history and not upon solid legal precedents.

Such complaints illustrate a common public misunderstanding of the permissible resources upon which the Court can draw in its work. They also call attention to the history profession's neglect of a vital area for which it is responsible, and about which it has done shamefully little in the last quarter century: American constitutional history and areas related to constitutional problems.

For the distressing aspect of Black's opinion is not that he used history. The Court has always used history;[2] the founding fathers expected it to do so.[3]

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[1] *Engle* v. *Vitale,* 370 US Reports, 421 (1962). On the general question of public attacks on the Court, see Arthur S. Miller, "A Note on the Criticisms of Supreme Court Decisions," *Journal of Public Law,* X (Spring 1961), 139–51.

[2] Edward S. Corwin, "The Constitution as Instrument and Symbol," *American Political Science Review,* XXX (Dec. 1936), 1072, indicates the long and heavy reliance of conservatives upon history, as "a useful device for the consecration of an already established order of things." This suggests that conservatives are not so concerned about the use of history as the purposes for which it is used.

[3] James Madison, writing late in his life of the Confederation and its faults, stated: "Such were the defects, the deformities, the diseases and the ominous prospects for which the convention were to provide a remedy, and which ought never to be overlooked in expounding and appreciating the Constitutional Charter, the remedy that was provided." (Cited in Robert L. Schuyler, *The Constitution of the United States: An Historical Survey of Its Formation* [New York, 1923], 92, and to which Professor Schuyler adds: "These words of the 'Father of the Constitution' sound like a protest against judicial interpretation of the Constitution by judges ignorant of its historical setting and, therefore, unable to fathom the original intent of its provisions. Unfortunately, a knowledge of American history has not yet been made a prerequisite for admission to the Supreme Court.")

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What should bother the historian is the type of history Black used in evolving a basis for an important constitutional ruling. For in tracing the historical development of church-state relations, the most recent work he cited was Thomas J. Wertenbaker's 1947 volume, *The Puritan Oligarchy,* and for the most part he relied upon works which may be "historical" given the length of time in the past they were written, but which modern scholars would hesitate to suggest an undergraduate rely upon as anything but a once important, although now outdated view.[4]

This is not so much an unfortunate reflection upon Black, however, as it is upon the historical profession because it points up the urgent need of its members to reclaim and revitalize this area. If we were doing a large body of outstanding research in constitutional development and it were being passed over for archaic sources, then we could legitimately criticize. Unfortunately, this is not and has not been the case for some years.

Why have we neglected constitutional history for so long? The reasons are complex, partially justifiable, and reflective of the tendency of our profession to respond to current needs and sometimes even current fads in the name of scholarly activity. One of the ironies of American historiography is that in the nineteenth century, when the American citizen was hardly touched in his everyday existence by his national government and very little by his state government, constitutional history tended to crowd out almost every other aspect of history. In the twentieth century, social, cultural, and intellectual history has risen in professional status to the rapid exclusion of constitutional, yet Americans have never been more regulated by government. Their social and economic life has increasingly taken on political and constitutional connotations until it has become difficult to point out any area of that life that is not restricted by law. Constitutional history's inability to attract adherents in such a favorable situation, then, must be a reflection, not on the subject matter, but on the nature of its approach, its style, and the

[4] E.g., on the English background of the question of church-state relations, Black relies solely upon Leighton Pullan, *The History of the Book of Common Prayer* (London and New York, 1900), and the 1957 edition of the *Encyclopedia Britannica;* on early American practice he uses Vernon Parrington's *Main Currents in American Thought* (3 vols., New York, 1927–30); Leon Whipple's *Our Ancient Liberties: The Story of the Origin and Meaning of Civil and Religious Liberty in the United States* (New York, 1927); and Thomas J. Wertenbaker, *The Puritan Oligarchy: The Founding of American Civilization* (New York, 1947). On the nature of the revolutionary established church controversy, he cites Sanford Cobb, *The Rise of Religious Liberty in America: A History* (New York, 1902); Charles F. James, *Documentary History of the Struggle for Religious Liberty in Virginia* (Lynchburg, Va., 1900); and John Fiske's *The Critical Period in American History* (Boston, 1888); and for Roger Williams he again uses Parrington, citing only his characterization of Williams as "the truest Christian amongst many who sincerely desired to be Christians." (*Engle* v. *Vitale,* 370 US Reports, 421 at 434 [1962].)

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materials that its practitioners have considered usable in its delineation. A look at its history makes this clear.

From its crude beginnings with George Bancroft to its traditional exposition by men such as Homer Hockett and Andrew C. McLaughlin, constitutional history was constrained and confined by a set of rigid assumptions. Because of the peculiar nature of the origin and development of the American constitutional system, there was a strong tendency for its students to be conservative and generally worshipful in their attitudes. This led them to adopt an approach toward the discovery of historical reality that might generously be called one of philosophic-metaphysical analysis geared to establish the validity of political orthodoxy, or more crudely might merely be referred to as "revealed" history to underwrite the virtue of established institutions. The subtlety and sophistication of the approach increased greatly as the cry arose for making history more scientific, yet the essential forms did not change. One started with a series of presumptions about the nature of God, the nature of God's universe, the nature of God's kingdom on earth, and man's proper role within this framework, and having established the correctness of these assumptions, then used them as a yardstick to measure man's behavior. Certainly with such knowledge, it was not difficult to evaluate what was moral, righteous, ethical, and decent behavior, who the right people were, or, by the same token, what was not to be considered proper. This, in turn, made historical inquiry a very simple process. It further made historical judgments relatively easy, if, at times, somewhat rigid.

Bancroft, for all his avid interest in pursuing source materials, found it easy and natural to take the position in his writings that our democracy and constitutional government were God-given—manifestations of the will of Divine Providence. If not responsible for originating the long-accepted myth of the semidivine character of the founding fathers, he did nothing to discourage its full acceptance and created for later scholars the danger of seeming mildly heretical if they openly rejected it.

Then, with the cessation of hostilities in 1865, an attempt was quickly made by both northern and southern historians to wrap a shield of historical orthodoxy and justification around both sections' constitutional position and political leadership in the late secessionist crisis. Principal gospelgiver for the South was former Vice-President of the Confederacy, Alexander H. Stephens, whose two-volume work, *The Constitutional View of the Late War between the States* (Philadelphia, 1868–70), contained a complete constitutional justification for secession argued in terms of enlightened southern leadership correctly supporting state sovereignty and the true preservation

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of Grecian civilization. He was subsequently joined by Jefferson Davis, whose *The Rise and Fall of the Confederate Government* (New York, 1881), although later, gave no ground on constitutional questions. The northern inspired 128-volume, *Official Records of the Union and Confederate Armies* (Washington, D. C., 1880–1901), conspicuously subtitled *The War of the Rebellion,* by that very term suggested that secession was not legal under our form of government and that southerners had violated constitutional forms and agreements, and by implication the natural and moral order of things, by defying federal authority. Less dispassionate were various popular writers of the day, principally Horace Greeley[5] and Henry Wilson.[6] Wilson, especially, drew northern popular acclaim, but he also aroused some northern scholarly distress, in arguing that the dissolution of the Union was solely the result of a thirty-year conspiracy on the part of southerners led by the detestable rebel John C. Calhoun, who from the 1830's had hidden behind the metaphysical shield of devious constitutional theories as he plotted the destruction of the nation.

Herman Eduard von Holst, probably the first outstanding academic constitutional historian, lent scholarly veneer to the Greeleys and the Wilsons in his seven-volume *Constitutional and Political History of the United States* (Chicago, 1874–92). Utilizing the tedious and ponderous techniques innate in his German training, von Holst argued that at the beginning of our nation under the Constitution the framers had intended no such thing as state sovereignty, the states having relinquished any sovereignty they had earlier when they signed the Articles of Confederation. This, plus his almost irrational hatred of slavery, led him to suggest that immoral southerners had evolved the state sovereignty argument as an illegal defense of an institution in itself illegal. The only happy side of the American past was the victory of the northern forces of light over this heinous conspiracy. Less violent but equally firm in his antislavery convictions was the legally oriented James Schouler, whose seven-volume *History of the United States of America under the Constitution* (Washington, D. C., and New York) reached the public irregularly between 1880 and 1913.

But the methodology of nineteenth-century history generally is also relevant. Historians of the time were committed to an institutional-legal approach to the study of the past, an approach that beautifully complemented the study of the Constitution, but that, as early as the 1880's, was already inducing

[5] Horace Greeley, *The American Conflict: A History of the Great Rebellion in the United States of America, 1860–64* (2 vols., Chicago, 1865–66).

[6] Henry Wilson, *History of the Rise and Fall of the Slave Power in America* (3 vols., Boston, 1876–78).

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certain expansive minds to call for emancipation. This approach, defining institutions as forms of social behavior primarily definable by law, confined legitimate areas of inquiry to the origins and continuity of such institutions. This sent would-be historical scholars probing into the past, highly restricted both as to the type of questions they could ask and the type of evidence they could use to construct pertinent answers. The suggestion, for example, that one might study an individual judge in an attempt to learn whether his personal predilections might have affected constitutional development would have been considered both unbecoming and unhistorical. Those who would study the Constitution should especially heed Edmund Burke's suggestion regarding the English constitution: "We ought to understand it according to our measure; and to venerate where we are not able to understand."

By the 1890's the movement to break out of such strictures was gaining apace. Frederick Jackson Turner's suggestion that institutions did not simply evolve within their own set patterns and that external forces could decisively influence their course, while not immediately destroying a legal-institutional approach to historical development, at least suggested that broader areas of inquiry were permissible in seeking to reconstruct the past. Yet Turner and his historical successors were merely part of a scholarly revolution that was taking place generally within the social sciences beginning around the turn of the century. The pragmatic revolt in scholarship, involving a new sophistication and healthy skepticism toward rigid ancient truths and an empirical-rational approach toward the determination of reality, opened the door to the restless and hyperinquisitive historical scholar to begin asking questions previously felt unanswerable. And with the increasing impact of the ideas of Marx, Darwin, Dewey, and Freud, permissible areas of inquiry were extended even further. Yet the response of students of the Constitution was, in all but a few instances, disappointing. Charles A. Beard, certainly not unimpressed with the fact that the law itself was beginning to reflect the new emphasis pressed upon it by advocates of sociological jurisprudence, did try his hand at constitutional reassessment. In re-evaluating the actions of the founding fathers, Beard utilized the analysis of ideas, social forms, and economic needs. But while not as doctrinaire as his Marxist contemporaries, A. M. Simons and Gustavus Myers, he operated on the assumption that to determine primary economic motivations would prove the most revealing avenue for ultimate reality.

Yet contemporary scholars of the Constitution did not follow Beard's lead. The reasons are clear if not totally warranted. Beard had departed sharply and frankly (and unjustifiably) from the traditional set of assumptions about constitutional development. It was inconceivable, that given the virtue

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of the Constitution and our constitutional system and the type of men the founding fathers were known to have been, that petty human motives, especially those of personal aggrandizement, could have been present in its origin. The type of inquiry Beard was making and even the facts that the inquiry produced were not, they thought, relevant types of information for studying the past. But equally present was the fact that Beard as well as Myers, Simons, and John R. Commons, who wrote *Legal Foundations of Capitalism* (New York, 1924), were not constitutional historians; nor were they interested in writing constitutional history per se. They were interested in analyzing the Constitution and the Supreme Court as a way of finding answers about nonconstitutional areas of American life. As such, the type of information that they put forth was not highly useful to either students of the history of explicit aspects of the law or the development of legal precedents, or to lawyers or judges seeking information on the historical antecedents of explicit modern institutions and practices.

Thus despite the revolution in knowledge and the approach to it fostered by the pragmatic revolt, traditional constitutionalists felt themselves justified in adhering to fixed and ancient patterns. Such a tradition was preserved in the two most well-known works in "pure" constitutional history in the first quarter of the twentieth century, Albert J. Beveridge's four-volume *Life of John Marshall* (New York, 1916–19) and Charles Warren's three-volume *The Supreme Court in United States History* (Boston, 1923). While both Beveridge and Warren were nonacademicians, the academic constitutionalists still plying their trade were apparently inspired more by their success than by the example of Beard's heresy and the condemnation it brought. This does not suggest that the steadily decreasing number of such scholars did not feel compelled to adopt more sophisticated techniques in their scholarship. Certainly the few remaining active constitutional historians, McLaughlin, Hockett, J. G. Randall, conducted their research within a self-conscious frame of scientific historical analysis. Yet the result was seldom more than a far more sophisticated version of nineteenth-century legal-institutional "revealed" history, in a modern package. By the 1930's the literature of American constitutional history revealed both that its old practitioners were largely talking only to each other and that, with exceptions, the profession generally had rejected the discipline for other historical approaches and fields. One immediate result was to create a temporary vacuum. The vacuum, however, was quickly filled by a number of lawyers, political scientists, and journalists who assumed the responsibility for doing what research and writing were still being produced in the area.[7]

[7] Typical of such works are Walton H. Hamilton and Douglass Adair, *The Power to Govern: The Constitution Then and Now* (New York, 1937); Robert H. Jackson, *The Struggle for*

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Fortunately, in light of this rejection of responsibility by historians, those constitutional lawyers in political science departments and lawyer-historians who attempted to keep the field alive were not generally bound by the constraints that historians had seemed unable to break. The years between the late 1930's and the 1950's saw much of merit coming from them.

Benjamin F. Wright, who had earlier explored the role of natural law in American constitutional development,[8] made valuable contributions with a work on the contract clause in 1938[9] and a pithy and perceptive summary of *The Growth of American Constitutional Law* in 1942.[10] Carl B. Swisher, with revealing studies of Field and Taney, showed the valuable potentials of judicial biography.[11] This area was one in which much other useful work was done. Historians, peculiarly and inexcusably, left it almost entirely alone.[12]

Judicial Supremacy: A Study of a Crisis in American Power Politics (New York, 1941); Fred Rodell, *Fifty Five Men* (New York, 1936); Felix Frankfurter, *The Commerce Clause under Marshall, Taney and Waite* (Chapel Hill, N.C., 1937); Beryl H. Levy, *Our Constitution: Tool or Testament?* (New York, 1941); Robert K. Carr, *The Supreme Court and Judicial Review* (New York, 1942); Edward S. Corwin, *Twilight of the Supreme Court* (Princeton, N. J., 1934), *The Commerce Power versus States Rights* (Princeton, N. J., 1936), *Court over Constitution: A Study of Judicial Review as an Instrument of Popular Government* (Princeton, N. J., 1938), *The President: Office and Powers* (New York, 1940), and *Constitutional Revolution, Ltd.* (Claremont, Calif., 1941); Carl B. Swisher, *American Constitutional Development* (Boston, 1943); Joseph Alsop and Turner Catledge, *The 168 Days* (Garden City, N. Y., 1938); Irving Brant, *Storm over the Constitution* (Indianapolis, 1936); Drew Pearson and Robert Allen, *The Nine Old Men* (New York, 1937); and Merlo J. Pusey, *The Supreme Court Crisis* (New York, 1937). This is not to infer that no constitutional history was being written by historians. The 1930's saw the publication of two textbooks in the field: Homer Hockett, *The Constitutional History of the United States, 1776–1876* (2 vols., New York, 1939), and Andrew C. McLaughlin, *A Constitutional History of the United States* (New York, 1935). Each was highly traditional, and McLaughlin, like Hockett, paid relatively little attention to developments following 1885. The decade also brought important studies such as Lawrence A. Harper, *The English Navigation Acts: A Seventeenth Century Experiment in Social Engineering* (New York, 1939); Richard B. Morris, *Studies in the History of American Law* (New York, 1930); and Lewis G. Van der Velde, *The Presbyterian Churches and the Federal Union, 1861–1869* (Cambridge, Mass., 1932). It is nonetheless significant that in the 1938 collection of essays commemorating the 150th anniversary of the Constitution, *The Constitution Reconsidered*, ed. Conyers Read (New York, 1938), only two of the twenty-eight contributors were active practitioners of American constitutional history.

[8] Benjamin F. Wright, *American Interpretations of Natural Law* (Cambridge, Mass., 1931).

[9] *Id., The Contract Clause of the Constitution* (Cambridge, Mass., 1938).

[10] *Id., The Growth of American Constitutional Law* (New York, 1942).

[11] Carl B. Swisher, *Stephen J. Field, Craftsman of the Law* (Washington, D. C., 1930), and *Roger B. Taney* (New York, 1935).

[12] Worth noting in the area of judicial biography are: Francis P. Weisenburger, *The Life of John McLean: A Politician on the United States Supreme Court* (Columbus, Ohio, 1937); Bruce R. Trimble, *Chief Justice Waite—Defender of the Public Interest* (Princeton, N. J., 1938); Beryl Levy, *Cardozo and Frontiers of Legal Thinking* (New York, 1938); George S. Hellman, *Benjamin N. Cardozo: American Judge* (New York, 1940); Marie Carolyn Klinkhamer, *Edward Douglas White: Chief Justice of the United States* (Washington, D. C., 1943); Alexander A. Lawrence, *James Moore Wayne: Southern Unionist* (Chapel Hill, N. C., 1943); Joseph E. McLean, *William Rufus Day* (Baltimore, 1947); Willard King, *Melville Weston Fuller: Chief Justice of the United States* (New York, 1950), and *Lincoln's Manager, David Davis* (Cambridge, Mass., 1960); Charlotte Williams, *Hugo L. Black: A Study in the Judicial Process* (Baltimore, 1950); Samuel Hendel, *Charles Evans Hughes and the Supreme Court* (New York, 1951); Joel F. Paschal, *Mr. Justice Sutherland: A Man against the State* (Princeton, N. J., 1951); George Shiras, *Justice George Shiras, Jr., of Pittsburgh, Associate Justice of the Supreme Court, 1892–*

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Charles G. Haines, avoiding both the incensement and tendency to produce a lawyer's brief, which had marked the work of Louis Boudin,[13] brought out a useful corrective to the position of Charles Warren in his *Role of the Supreme Court in American Government and Politics* (Berkeley, Calif., 1944). Wallace Mendelson raised a series of important questions about the Dred Scott case and added to historians' knowledge of the evolution of due process.[14] Alpheus T. Mason shamed historians who had apparently feared the legal involvements of twentieth-century constitutional subjects by a long series of historically oriented works, as also did Samuel Konefsky.[15] Edward S. Corwin continued both to make valuable contributions and to inspire his political science graduates to do likewise.[16] Robert McCloskey and C. Herman Pritchett produced generally well-received works on, respectively, nineteenth-century political-judicial thought,[17] and the Roosevelt, Vinson, and Warren Courts.[18] Law professors such as Charles Fairman,[19] Mark DeWolfe

1903 (Pittsburgh, 1953); Henry Steele Commager, "Joseph Story," in *Bacon Lectures on the Constitution of the United States, 1940–1950* (Boston, 1953), 33–94; Donald Morgan, *Justice William Johnson: The First Dissenter* (Columbia, S. C., 1954); Charles Page Smith, *James Wilson, Founding Father, 1742–1798* (Chapel Hill, N. C., 1956); for Howe's work on Holmes, see note 20, below; Leonard W. Levy, *The Law of the Commonwealth and Chief Justice Shaw* (Cambridge, Mass., 1957); Eugene C. Gerhart, *Robert H. Jackson, America's Advocate* (Indianapolis, 1958); H. Landon Warner, *The Life of Mr. Justice Clarke* (Cleveland, 1959); Helen S. Thomas, *Felix Frankfurter, Scholar on the Bench* (Baltimore, 1960); Wallace Mendelson, *Justices Black and Frankfurter: Conflict in the Court* (Chicago, 1961); Robert K. Newmeyer, "Justice Joseph Story—A Political and Constitutional Study," unpublished doctoral dissertation, University of Nebraska, 1957; Lewis Maddocks, "Justice John M. Harlan," unpublished doctoral dissertation, Ohio State University, 1959; and Stephen R. Mitchell, "Mr. Justice Horace Gray," unpublished doctoral dissertation, University of Wisconsin, 1960. Worth noting also is that of this group only three, Commager, Klinkhamer, and L. Levy, are primarily constitutional historians.

[13] Louis B. Boudin, *Government by Judiciary* (2 vols., New York, 1932).

[14] Wallace Mendelson, "Dred Scott's Case—Reconsidered," *Minnesota Law Review,* XXXVIII (Dec. 1953), 16–28, and "A Missing Link in the Evolution of Due Process," *Vanderbilt Law Review,* X (Dec. 1956), 125–37.

[15] Alpheus T. Mason, *Organized Labor and the Law* (Durham, N. C., 1925), *Brandeis: Lawyer and Judge in the Modern State* (Princeton, N. J., 1933), *Brandeis: A Free Man's Life* (New York, 1947), *Harlan Fiske Stone: Pillar of the Law* (New York, 1956), *The Supreme Court from Taft to Warren* (Baton Rouge, La., 1958); Samuel J. Konefsky, *Chief Justice Stone and the Supreme Court* (New York, 1945), *The Constitutional World of Mr. Justice Frankfurter* (New York, 1949), *The Legacy of Holmes and Brandeis: A Study in the Influence of Ideas* (New York, 1956).

[16] Edward S. Corwin, *Total War and the Constitution* (New York, 1947), *Liberty against Government* (Baton Rouge, La., 1948), and *A Constitution of Powers in a Secular State* (Charlottesville, Va., 1951); Clinton Rossiter, *Constitutional Dictatorship: Crisis Government in the Modern Democracies* (Princeton, N. J., 1948), *The Supreme Court and the Commander in Chief* (Ithaca, N. Y., 1951), and *Seedtime of the Republic: The Origin of the American Tradition of Political Liberty* (New York, 1953); and Benjamin R. Twiss, *Lawyers and the Constitution: How Laissez Faire Came to the Supreme Court* (Princeton, N. J., 1942).

[17] Robert McCloskey, *Conservatism in an Age of Enterprise: A Study of William Graham Sumner, Stephen J. Field and Andrew Carnegie* (Cambridge, Mass., 1951).

[18] C. Herman Pritchett, *The Roosevelt Court: A Study in Judicial Politics and Values* (New York, 1948), *Civil Liberties and the Vinson Court* (Chicago, 1954), *The Political Offender and the Warren Court* (Boston, 1958); and *Congress versus the Supreme Court* (Minneapolis, 1961).

[19] Charles Fairman, *Mr. Justice Miller and the Supreme Court, 1862–1890* (Cambridge, Mass., 1939), "The So-Called Granger Cases, Lord Hale and Justice Bradley," *Stanford Law Review,* V (July 1953), 587–679, and "What Makes a Great Justice? Mr. Justice Bradley and the

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Howe,[20] Zechariah Chafee,[21] Julius Goebel,[22] Jacobus Ten Broek,[23] Bernard Schwartz,[24] and Howard J. Graham[25] also shouldered historical burdens. And, as a demonstration that the subject matter of constitutional development did not lack popular appeal, Merlo Pusey's biography of Charles Evans Hughes won the Pulitzer Prize for biography in 1952, while Catherine Drinker Bowen's jejune biography of Justice Oliver Wendell Holmes, a choice of the Book-of-the-Month Club, made best seller lists and eventually the theaters in an even less satisfying grade *B* movie.[26]

Historians were frequently aroused to strong and just criticism of the efforts of nonhistorians to produce such historical works. Much of the writing of Corwin and his students was present-minded and done as a response to current crises. Clinton Rossiter's *Seedtime of the Republic,* although it won the Bancroft Prize for history, distressed many historians as little more than a compilation of well-known material valuable largely in that it brought together in one place much useful information. A. T. Mason, while highly knowledgeable in law and judicial procedure, drew fire for his distressing lack of internal consistency and his questionable use of material, especially in his biography of Stone,[27] and also for his failure to set his figures in anything but a judicial world, despite their frequent ventures into other environments. Konefsky sinned similarly, while one reviewer candidly branded the second volume of Charles G. Haines's work (completed by Foster H. Sherwood) a "stale rehash"[28] and another pointed out McCloskey's naïveté in failing to understand that Andrew Carnegie relied heavily upon the ghost writer James H. Bridge and that it is dangerous to use the writings

Supreme Court, 1870–1892," in *Bacon Lectures on the Constitution of the United States, 1940–1950* (Boston, 1953), 423–85.

[20] Mark DeWolfe Howe, *Readings in American Legal History* (Cambridge, Mass., 1949), and *Justice Oliver Wendell Holmes: The Shaping Years* (Cambridge, Mass., 1957).

[21] Zechariah Chafee, *Free Speech in the United States* (Cambridge, Mass., 1941), *How Human Rights Got into the Constitution* (Boston, 1952), *Three Human Rights in the Constitution of 1787* (Lawrence, Kan., 1956), and *The Blessings of Liberty* (Philadelphia, 1956).

[22] Julius Goebel and T. Raymond Naughton, *Law Enforcement in Colonial New York, 1614–1776* (New York, 1944).

[23] Jacobus Ten Broek, *The Anti-Slavery Origins of the Fourteenth Amendment* (Berkeley, Calif., 1951).

[24] Bernard Schwartz, *The Supreme Court: Constitutional Revolution in Restrospect* (New York, 1957).

[25] Howard J. Graham, "The Conspiracy Theory of the Fourteenth Amendment," *Yale Law Journal,* XLVII, XLVIII (Jan., Dec. 1938), 371–403, 171–94, "Our 'Declaratory' Fourteenth Amendment," *Stanford Law Review,* VII (Dec. 1954), 3–39, "Builded Better Than They Knew—The Framers, the Railroads and the Fourteenth Amendment," *University of Pittsburgh Law Review,* XVII (Summer 1956), 537–84.

[26] Merlo J. Pusey, *Charles Evans Hughes* (2 vols., New York, 1951); Catherine Drinker Bowen, *Yankee from Olympus: Justice Holmes and His Family* (Boston, 1944).

[27] Among others, see my review in *Western Political Quarterly,* XI (June 1958), 413–15.

[28] Review by Leonard W. Levy in *American Historical Review,* LXIII (Apr. 1958), 696.

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of any business leader for analysis either of his mind or his times.[29] Pusey's *Charles Evans Hughes,* which voted against Franklin D. Roosevelt on virtually every page and ignored large segments of that leader's public career, and Mrs. Bowen's *Yankee from Olympus* were frequently either ignored or taken lightly for what they were, undisciplined and subjective narratives. Historians, however, were in a poor position to be too hostile since they were doing so little themselves in the constitutional area.

The luxury of having someone do one's job and then caviling at his efforts, however, began to be undercut in the 1950's as political scientists' interest waned and shifted. With the rise especially of what Dwight Waldo in 1954 was to call "the behavioral vogue," and the tendency, as Waldo further pointed out "of the larger foundations to support empirical study in the social sciences, as against more traditional studies,"[30] political scientists turned rapidly away from the constitutional area. The transition, while not without its valuable sides, especially in pointing out new ways to look at and assess judicial behavior, initially aroused concern among more traditional political scientists. V. O. Key and Frank J. Munger in 1954 questioned the inroads that the behaviorists were making because of their tendency to sublimate totally the historical content of political and constitutional behavior.[31] By 1961 William Beaney began a review of James Willard Hurst's *Law and Social Process in United States History* with the statement: "Because of the profession's diminished interest in law and its indifference toward history, it is unlikely that many political scientists will read this book."[32] Thus, if historians criticized their brethren for the use of Guttman scalogram analyses,[33] or Shapley-Shubik empirical power indexes,[34] or for creating psychometric models of the Supreme Court,[35] it was more apt to be on the grounds of skeptical bewilderment than improper use of history.

[29] Review by Fritz Redlich, *ibid.,* LVII (Apr. 1952), 707.

[30] Dwight Waldo, *Political Science in the United States of America* (Paris, 1956), 29. Waldo also includes a careful study, prepared by Gerald D. Nash, of articles in general political science journals for the periods 1925–1929, 1939–1941, and 1952–1954, showing, for example, that by the last period, public law and jurisprudence had declined to 7 per cent of the total of articles published by the profession; he concluded simply that "public law has been declining in attention, relatively speaking" (p. 46). See also Robert A. Dahl, "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest," *American Political Science Review,* LV (Dec. 1961), 763–72.

[31] Cited in *Handbook of Social Psychology,* ed. Gardner Lindzey (2 vols., Cambridge, Mass., 1954), II, 1125.

[32] Review by William Beaney in *American Political Science Review,* LX (Sept. 1961), 643.

[33] S. Sidney Ulmer, "Supreme Court Behavior and Civil Rights," *Western Political Quarterly,* XIII (June 1960), 288–311.

[34] Glendon A. Schubert, Jr., "The Study of Judicial Decision-Making as an Aspect of Political Behavior," *American Political Science Review,* LII (Dec. 1958), 1007–25.

[35] *Id.,* "A Psychometric Model of the Supreme Court," *American Behavioral Scientist,* V (Nov. 1961), 14–18.

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Yet, again, popular trends tended to move at crosscurrents with scholarly fads, for while public law specialists in political science departments were convincing themselves they were no longer historians and were really empirical behaviorists (and by so doing, as some critics have suggested, losing their professional identity) the use of history, as a key reliance in the shaping of public policy, rose to a new level of importance. This development was the product of the Supreme Court's new willingness to accept the role of public policy maker in a variety of congressionally stalemated areas of domestic development, combined with its traditional and well-accepted duty of construing the language used in the Constitution. This periodic supercession of Congress meant the utilization of different tools as the basis for public policy.

For Congress, while not unconcerned with history, sees its role largely in the present, with emphasis upon legislating for current needs and in the context of current problems. The Courts have traditionally fulfilled the function of construing established statutes and legal language. This is done in the context of both initial meaning and intent and later recasting, which has occurred when precedents have been inadequate to cope with the needs produced by changing situations. The judicial tradition, in other words, directs itself much more naturally than does the legislative to historical study—not merely to discover the precise locus of the productive language of, say, the Bill of Rights, but also to ascertain its thrust, its deep and enduring implications, and its over-all philosophical justification in a democratic society.

This development has occurred during the same time as the revolutionary constitutional changes that the last three decades have necessitated, a period in which the Court's role, while in many ways still traditional, took on an importance greater than at any previous time in American history. The rapid shift toward positive government and the leviathan state necessitated a complete reassessment of all the clauses of the Constitution with politico-economic implications from commerce and taxing power to delegation of executive authority and the Tenth Amendment. The complete reassessment of the relationship between government and the individual as related to personal liberties, which has also dominated our times, has not only brought constitutional change but the assurance of the current Chief Justice that over the next few years virtually every clause in the Bill of Rights will have to be reinterpreted. To reinterpret for the Court means to reassess the logical extension of first principles. In such a process not only will history play a vital role, but a new role as an auxiliary tool for the jurist, not for "the consecration of an already established order of things," but for a new order seeking a new level of equal rights and social justice through law.

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When in 1925 the Court took its first faltering but significant steps toward assuming an activist role (in this instance by laying the groundwork for the subsequent "nationalization" of the Bill of Rights by suggesting that certain of those guarantees might be enforceable against the states through a return to the original meaning of the Fourteenth Amendment),[36] a turn to history was essential. This trend was brought to fruition in 1931 in cases involving freedom of speech,[37] and freedom of the press,[38] and later extended by application to freedom of assembly in 1937,[39] and the "free exercise" section of the religious provisions of the First Amendment with a long series of Jehovah's Witnesses' rulings between 1938 and 1946. These cases necessitated a clear reassessment both of the actual intent of the framers of that amendment and the development and rationale of fifty years of erosion of the amendment as a device for the protection of individual rights. Conversely, the Court, starting with the O'Gorman case of 1931,[40] and going on through Nebbia in 1934,[41] and Stone's famous 1938 Carolene footnote (stating the "preferred freedoms" concept of the supercession of individual rights over property rights in cases involving state police power),[42] wiped much of the antiregulatory sheen off the due process clause. And while not agreeing that it should confess its historic sins of ever having taken this interpretive turn in the first place (as new Justice Hugo Black urged in 1938),[43] the Court nonetheless made clear that it was willing to return, as well as it could in conformity with good judgment and *stare decisis,* to an earlier day. Similarly, starting in 1931 the Court took the activist position in reassessing certain of the procedural guarantees of the Bill of Rights. This trend was less clear cut and frequently involved backtracking. Cardozo, for example, in 1937 seriously questioned the advisability

[36] *Gitlow* v. *New York,* 268 US Reports 652 (1925).

[37] *Stromberg* v. *California,* 283 US Reports 359 (1931).

[38] *Near* v. *Minnesota,* 283 US Reports 697 (1931). On the historical background of the Near case, see John E. Hartmann, "The Minnesota Gag Law and the Fourteenth Amendment," *Minnesota History,* XXXVII (Dec. 1960), 161–73.

[39] *DeJonge* v. *Oregon,* 299 US Reports 353 (1937).

[40] *O'Gorman & Young, Inc.,* v. *Hartford Ins. Co.,* 282 US Reports 251 (1931). On the importance of the O'Gorman case, see Harry Shulman, "The Supreme Court's Attitude toward Liberty of Contract and Freedom of Speech," in *Selected Essays on Constitutional Law* (4 vols., Chicago, 1938), II, 1098–1106.

[41] *Nebbia* v. *New York,* 291 US Reports 502 (1934). In his majority opinion Justice Owen J. Roberts ruled that there was no class of business which was not "clothed with a public interest," thereby removing a historic hurdle from the path of broad-scale state regulation of economic activities.

[42] *United States* v. *Carolene Products Co.,* 304 US Reports 114 (1938). The footnote, although not having the force of law, nonetheless marked the first frank acknowledgment from the bench that in the future the justices would take a much longer and more critical look at state restrictions upon individual liberties than upon economic activities.

[43] *Connecticut General Life Ins. Co.* v. *Johnson,* 303 US Reports 77 (1938). For a discussion of the implications of Black's suggestions and their historical setting, see Andrew C. McLaughlin, "The Court, the Constitution and Conkling," *American Historical Review,* XLVI (Oct. 1940), 45–63.

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of an absolutist position with regard to the guarantees of the Second through Eighth Amendment. He also suggested at the same time, however, that the Court might make it its business to activate the Bill of Rights in new areas "implicit in the concept of ordered liberty."[44] This suggestion found picketing redefined as free speech in 1940[45] and similar "sacredness" placed around labor union organization in 1944.[46]

In other areas, history became a vital reliance for sweeping changes of public policy. Justice George Sutherland in 1936, relying upon a shockingly inaccurate use of historical data concerning the original transferal of power from the Continental Congress to the new government under the Constitution, laid the constitutional basis for virtual plenary executive authority in the area of foreign relations.[47] Having previously avowed his belief while Attorney General that utilization of the commerce clause as the essential device for the regulation of virtually every aspect of the economy was actually carrying out the initial intentions of John Marshall,[48] Justice Robert H. Jackson wrote that position into judicial precedent in 1943, with a long historical discussion documenting more fully its evolution.[49]

More recent times have found the Court returning to the "true meaning" of the Fifteenth Amendment in eliminating a great variety of state restrictions upon voting rights, from grandfather clauses,[50] and white primaries,[51] to unequal and unmodernized districting procedures.[52] More familiarly it has erased the 1896 "separate but equal" gloss on the Fourteenth Amendment's equal protection clause, thereby eliminating over fifty years of obstacles to the equality before the law that the framers of the amendment had sought to guarantee.[53]

[44] *Palko* v. *Connecticut*, 302 US Reports 319 (1937).

[45] *Thornhill* v. *Alabama*, 310 US Reports 88 (1940).

[46] *Thomas* v. *Collins*, 323 US Reports 516 (1945). Despite its heavy reliance on history in reassessing the Fourteenth Amendment, as late as 1949, three of the justices confessed they were reading H. E. Flack, *Adoption of the Fourteenth Amendment* (Baltimore, 1908), as the historical authority on the amendment. (Walter P. Armstrong, "What Do the Justices Read? Books of Interest to Supreme Court Members," *American Bar Association Journal*, XXXV (Apr. 1949), 295–300.

[47] *United States* v. *Curtiss-Wright Export Corp.*, 299 US Reports 304 (1936).

[48] Robert H. Jackson, *The Struggle for Judicial Supremacy* (New York, 1941); see also Robert L. Stern, "The Commerce Clause and the National Economy, 1933–1946," *Harvard Law Review*, LIX (May, June 1946), 645–93, 883–947; and Paul L. Murphy, "The New Deal and the Commerce Clause," unpublished doctoral dissertation, University of California, 1953.

[49] *Wickard* v. *Filburn*, 317 US Reports 111 (1942).

[50] *Lane* v. *Wilson*, 307 US Reports 268 (1939).

[51] *United States* v. *Classic*, 313 US Reports 299 (1941); *Smith* v. *Allwright*, 321 US Reports 649 (1944); *Terry* v. *Adams*, 345 US Reports 461 (1953).

[52] *Baker* v. *Carr*, 369 US Reports 186 (1962).

[53] Although it can only be speculation, an interesting question is raised as to whether the subsequent public castigation of the Court for relying upon the "modern authority" of six sociological and psychological texts in rendering the famed 1954 integration ruling, *Brown* v. *Board of Education*, 347 US Reports 483 (1954), may not have had the effect of pushing it

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In virtually all of these rulings, the Court, when it cited history, either relied upon archaic historical works of the earlier devotees of "revealed" history, turned to history written by nonhistorians, or trusted its own ability to reconstruct historical evidence from the sources themselves.[54] The fact should disturb sensitive historians. Yet it should also force them to reassess the way the Court uses history and should challenge them to examine carefully what steps they can take to improve the situation.

History, to the legal profession—"law office history," as Howard J. Graham has frequently called it—has a peculiarly functional quality as an aspect of legal advocacy. The law clerk assigned to provide historical materials for the bolstering of a brief or an opinion knows that his job is to find those materials which will best serve to persuade the judges to rule favorably in the immediate case. That he may choose his evidence from only one side, or in such a way as partially to distort the record, will neither cause him qualms nor greatly distress the judge. The ultimate object, that of gaining a certain legal determination, is the focus. "Law office history," then, is not only deliberately calculated to win cases, but its inadequacy as academic history is never questioned by the bench and bar. As Saul Touster has so well pointed out, "The questions asked, the values expressed, and the factors considered in a law-making context are not the same as those present in a research context."[55]

Granted, then, there is little that the history profession can do to change the methodology of legal decision making. And with the Court using history, even if in nonacademic fashion, to attack many of the legal myths of a static society and thereby reach new and significant plateaus of social and economic justice, there is even less incentive to try. Yet, the historian can take positive steps that can be of relevance. If the Court is intent upon building new and dramatic legal structures to meet the requirements of a dynamic

back toward the use of history and precedent in subsequent rulings, especially those calculated to arouse strong public emotions. (See Mason, *Supreme Court from Taft to Warren,* Chap. VI.) The Court certainly had adequate historical information at hand, since historian Alfred H. Kelly had compiled thorough background information which was embodied in the appellants' brief on reargument. (See Alfred H. Kelly, "The Fourteenth Amendment Reconsidered—The Segregation Question," *Michigan Law Review,* LIV [June 1956], 1049–86.)

[54] Justice Harold H. Burton told an interviewer in 1953: "The Justices of the Supreme Court do take judicial notice of historical facts. We usually go back to original sources for our information rather than looking into secondary historical writings." He, himself, he stated, "went way back into colonial times" to investigate certain historical facts that illuminate issues of a case under study. (John J. Daly, *The Use of History in the Decisions of the Supreme Court: 1900–1930* [Washington, D. C., 1954], ix.) For a current reminder of the danger inherent in writing history from pure source material, see the wise comments of C. Vann Woodward, "The Anti-Slavery Myth," *American Scholar,* XXXI (Spring 1962), 318 ff.

[55] Saul Touster, "Law and Psychology: How the Twain Might Meet," *American Behavioral Scientist,* V (May 1962), 3–4.

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society, the historian can at least furnish it with complementary modern architectural materials, so that it does not have to rely upon scrap lumber, salvage bricks, and raw stones for its buildings.

What the profession can profitably and easily do is to accept the responsibility that the new judicial emphasis, and the declining interest of political scientists, forces upon it. Despite the labors of a few brave pilgrims in recent years,[56] vast areas of constitutional and constitutionally related subject matter, some of which have scarcely been touched for decades, lie crying for attention. Further, if these areas were subjected to the rigorous and modern research techniques that historians and social scientists generally have perfected in the last decade, their results might well be startling.

Further, the opportunity is a unique and exciting one. With the Courts more and more shaping public policy, the opportunity to play a coordinate

[56] In 1948 Alfred H. Kelly and Winfred A. Harbison published the first textbook in the field to treat thoroughly constitutional development from English antecedents to the present, *The American Constitution: Its Origins and Development* (New York, 1948); see also note 7, above. The year 1958 saw the publication of James M. Smith and Paul L. Murphy, *Liberty and Justice: A Historical Record of American Constitutional Development* (New York), the first reading collection devoted solely to constitutional history since the companion set of Allen Johnson, *Readings in American Constitutional History, 1776–1876* (Boston, 1912); and *id.* and William A. Robinson, *Readings in Recent American Constitutional History, 1876–1926* (New York, 1927). Special studies of note coming from historians included: Edmund S. Morgan and Helen M. Morgan, *The Stamp Act Crisis: Prologue to Revolution* (Chapel Hill, N. C., 1953), and Edmund S. Morgan, *Prologue to Revolution: Sources and Documents on the Stamp Act Crisis, 1764–1766* (Chapel Hill, N. C., 1959); Harold M. Hyman, *Era of the Oath: Northern Loyalty Oaths during the Civil War and Reconstruction* (Philadelphia, 1954) and *To Try Men's Souls: Loyalty Tests in American History* (Berkeley, Calif., 1959); James M. Smith, *Freedom's Fetters: The Alien and Sedition Laws and American Civil Liberties* (Ithaca, N. Y., 1956); Robert E. Brown, *Charles A. Beard and the Constitution* (Princeton, N. J., 1956); David M. Silver, *Lincoln's Supreme Court* (Urbana, Ill., 1957); Robert S. Hunt, *Law and Locomotives: The Impact of the Railroad on Wisconsin Law in the 19th Century* (Madison, Wis., 1958); Winton U. Solberg, *The Federal Convention and the Formation of the Union* (New York, 1958); Forrest McDonald, *We the People: The Economic Origins of the Constitution* (Chicago, 1958); Leonard W. Levy, *Legacy of Suppression: Freedom of Speech and Press in Early American History* (Cambridge, Mass., 1960); Arnold Paul, *Conservative Crisis and the Rule of Law* (Ithaca, N. Y., 1960); Jackson T. Main, *The Antifederalists: Critics of the Constitution* (Chapel Hill, N. C., 1961); William R. Leslie, "The *Gaspée* Affair: A Study of Its Constitutional Significance," *Mississippi Valley Historical Review,* XXXIX (Sept. 1952), 233–56, and "Similarities in Lord Mansfield's and Joseph Story's View of Fundamental Law," *American Journal of Legal History,* I (July 1957), 278–307; Kelly, "Fourteenth Amendment Reconsidered"; Arthur Bestor, "State Sovereignty and Slavery," *Journal of the Illinois State Historical Society,* LIII (Summer 1961), 117–80; Stanley I. Kutler, "Labor, the Clayton Act, and the Supreme Court," *Labor History,* III (Winter 1962), 19–38, and "Chief Justice Taft, Judicial Unanimity and Labor," *Historian,* XXIV (Nov. 1961), 68–83; Everett Swinney, "Enforcing the Fifteenth Amendment, 1870–1877," *Journal of Southern History,* XXVIII (May 1962), 202–18; Leonard W. Levy, "Liberty and the First Amendment: 1790–1800," *American Historical Review,* LXVIII (Oct. 1962), 22–37, and *id.* and Lawrence H. Leder, " 'Exotic Fruit': The Right against Compulsory Self-Incrimination in Colonial New York," *William and Mary Quarterly,* 3d Ser., XX (Jan. 1963), 3–32. In their article, Levy and Leder respond vigorously to the barbs of Law Professor Julius Goebel concerning the inability of nonlawyers to write legal history (p. 5). The 1950's also saw the creation of the American Society for Legal History, with an organ, *The American Journal of Legal History,* and eventually even a Lawyer's Literary Club to furnish monthly selections of legal history to its members. Unfortunately the orientation of the movement was more one that sought to stimulate members of the bench and bar to try their hand in the historical area than to encourage academic participation in legal research.

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role through the furnishing of new historical materials, a role so often coveted and seldom realized by our profession, affords itself. Yet it does so without necessitating the compromise of professional standards by making our work present-minded. On the contrary, what is needed from historians is the most accurate, thoroughly documented, and impeccable history we are capable of producing. This certainly does not guarantee that the present Court will cease to be criticized when its decisions rely upon historical evidence for points of law, but should at least exempt us from the charge that we are not providing the Court with modern research findings, a charge that we cannot afford to ignore.

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Reviews of Books

General

GENERALIZATION IN THE WRITING OF HISTORY: A REPORT OF THE COMMITTEE ON HISTORICAL ANALYSIS OF THE SOCIAL SCIENCE RESEARCH COUNCIL. Edited by *Louis Gottschalk*. (Chicago: University of Chicago Press. 1963. Pp. xiii, 255. $5.00.)

THIS report of the Committee on Historical Analysis of the Social Science Research Council, unlike its predecessors, the well-known Bulletins 54 and 64 of that council, is published on the regular list of a university press. Like its predecessors, it is basically concerned with methodology; unlike them it is, save for occasional remarks by the committee's adviser from formal philosophy, Hans Meyerhoff, wholly the work of practicing historians. The six members of the committee, William O. Aydelotte, Thomas C. Cochran, Merle Curti, Roy F. Nichols, David M. Potter, and the editor, Louis Gottschalk, all make substantial contributions; in addition, Chester G. Starr, M. I. Finley, Arthur F. Wright, Derk Bodde, R. R. Palmer, and Walter P. Metzger supply articles or commentary. As Part III, there is an admirable "Bibliography of Writings on Historiography and the Philosophy of History" by Martin Klein, which brings up to date and supplements the familiar bibliography of Bulletin 54. Part I is devoted to a series of case studies of historical generalizations in special fields, ancient, Chinese, and modern, made by experts in those fields; Part II, the work of members of the committee, is a series of somewhat more theoretical (but never "abstract") essays on various aspects of the problem of historical generalization. Gottschalk concludes with a fine summary of the whole book; throughout he has supplied abundant cross references and notes that help tie the various contributions together.

For, though obviously it cannot have the unity of the work of a single mind, this symposium does not deserve the reviewer's stereotype for such collections: *uneven*. There is indeed a considerable range of attitude toward historical generalizations—or toward "theoretical history," a phrase that does crop up occasionally and would have horrified the founders of the American Historical Association. Of Gottschalk's six categories, "the school of the unique," the narrative-descriptive historians of very limited generalizations, the "school of generalization on the basis of trends," the comparative historians, the nomothetic historians, the outright "philosophers of history," most of the writing here is concerned with the third, fourth, and fifth. None of the contributors denies that the historian has

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to *make* generalizations; none hold, to quote Potter's summary description of the belief of the first generation of "scientific" historians, "that a body of data would reveal its own meaning and interpret itself, if only it were valid or authentic, and were arranged in time sequence." Potter himself, and indeed most of the contributors, insist that in practice all historians, including the generation that founded this *Review,* do something with their minds on the raw data ("facts") they gather.

Yet they are all aware, to quote Potter again, that the attitude of our predecessors toward historical method "still casts a long shadow." They are by no means on the defensive, apologetic, but they do realize that there is in the profession a strong current of distrust of theory, a firm belief that writing history is a craft, perhaps an art, and a science only in the sense that the historian must observe complete respect for facts, data, concrete "reality" and that he must seek for complete detachment, scientific objectivity. In brief, they know that in the profession there is a powerful complex of thought and feeling hostile to the social sciences as they stand today. Of the eleven contributors, only Metzger, and in his own field of economic history, Cochran, frankly use the working vocabulary and theoretical tools of a social science.

Some of this opposition of attitudes—social psychologists and sociologists would have varying terms ("labeling generalizations" in Gottschalk's words) to describe them—reflects a deep division among us all as human beings, a division I am willing to settle for in common-sense terminology as one between the narrative mind and the analytical mind. The dualism is imperfect and is cut across at various points by other dualisms, perhaps even by such old critical and philosophical chestnuts as "classic" and "romantic" or *Verstand* and *Vernunft,* or even "scientist" and "artist." In our time, the immense prestige and success of "pure" science in its collaboration with technology and business have reinforced the social esteem in which the analytical mind is held. Hence—and again one uses the vocabulary of this perhaps overworried age—the ambivalence many historians feel toward their role. I recall the late Professor Schumpeter, an analytical mind indeed, telling a graduate history club how much the economist would like to have better time series of prices over longer historical periods in order to cope with the problem of understanding the business cycle. He asked his audience to supply this need, whereat one of the young historians interjected, "Sir, you want us to be mere hewers of wood and drawers of water for you superior thinkers. Well, we want to be thinkers ourselves!"

Most of us, and certainly this committee, want to be thinkers ourselves. But thinking, even to this age of depth psychology, is a *conscious* effort with a technique that has to be labeled "theory." We need not give up our craft traditions; in fact, many of these writers stress heavily the importance of an *X*—Finley calls it "professionalism," and Potter writes of "sagacity and applied experience which cannot be taught as method"—an *X* that cannot be given an exact "quantified" formulation. But it does seem that the coming generation of historians will be

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obliged to meet squarely the problems brought out in this thoughtful book, and that it will produce more—and more respectable—comparative and nomothetic historians. Whether Clio will ever welcome the philosophers of history is more doubtful. And many of us would still settle for one good "narrative-descriptive historian" of the stamp of Admiral Morison or the late Garrett Mattingly in exchange for half a dozen nomothetic ones.

Harvard University CRANE BRINTON

PHILOSOPHY OF HISTORY AND THE PROBLEM OF VALUES. By *Alfred Stern*. (The Hague: Mouton & Co. 1962. Pp. 248. Glds. 25.)

THERE are three topics with which this book deals: the relations between history-as-actuality and history-as-known; attempts to find a pattern, or purpose, or law in the historical process; relativism and nonrelativism with respect to human values. Stern's method of treating these problems is to develop his own positive position through a preliminary confrontation of views held by others, commenting on what he finds lacking in each of them. No reader can fail to be impressed by the range of his references; as a consequence of his method, however, his own views do not stand out in sharp focus against the wealth of citations, and in the end they are simply espoused, not critically defended.

Stern has little sympathy with speculative philosophies of history, which attempt to find a pattern, purpose, or law in history: he believes that a critical inquiry into the nature of historical knowledge serves to "unmask" them. This is related to the fact that his own views regarding the nature of historical knowledge demand a sharp separation between history-as-actuality and history-as-known. In establishing this separation he relies less on the traditional arguments for it than he does upon citations from current continental discussions of historical experience. Stern has also been influenced by Kantian thought, and he seeks to show in what sense historical knowledge may be "objective" (in the sense of being "intersubjective"), without being "absolute." The purported solution seems to be that the categories by means of which historians articulate the primary experiences of the participants give an objective order to historical events, but that the further organization of these events in particular historiographical works are dominated by the historian's own perspective. Furthermore, Stern holds the familiar position that what determines the historian's perspective are evaluative factors. For this reason he, like others before him, comes to regard the problem of the basis and the conditioning of values as a crucial question for the theory of historical knowledge. It is with the problem of value that he is concerned in the second half of his book.

With respect to values, Stern rejects both historicism and the view that there are unchangeable natural rights. He proposes as an alternative the doctrine that "the human project of living requires certain objective conditions" and that there are, therefore, "existential values [which] . . . constitute supra-historical objective

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standards." These existential values he identifies as the values of human life and of health. The other values of man he does not regard as "transhistorical," but as dependent upon historically conditioned acts of will. The specific "projects" of different collective entities, such as nations, belong in this realm, and they give birth to different codes of values. All such codes are held to be transient: only the vital values of life and of health, which are taken to spring from what is unchangeable in the human condition, are held to be genuinely universal. And it is to these values alone that Stern turns in his attempt to overcome historicism.

I need scarcely point out that the position adopted in the second half of the book, whatever its merits or demerits as a theory of values, has little to do with the question of the nature and objectivity of historical knowledge. Like the theory of Rickert, which the author criticizes, but which his own view nonetheless resembles, Stern's book has failed to show the relations between "the logical, epistemological, and axiological conditions of historical knowledge"; only the axiological factors have been stressed. Furthermore, it would seem odd that one who has devoted his attention to the limitations of our knowledge of history-as-actuality should find no difficulties in establishing within a few pages (pp. 221–37) what are the specific collective "projects" that underlie the codes of values of four different nations, and that he should also not seem to find any methodological problems in the attempt to establish a theory of value itself.

Johns Hopkins University MAURICE MANDELBAUM

PHILOSOPHY AND HISTORY: A SYMPOSIUM. Edited by *Sidney Hook*. ([New York:] New York University Press. 1963. Pp. x, 403. $6.00.)

MUCH of this probing volume is devoted to the discussion of three theses: Morton White's, that historical narrative is chronicle plus "because" statements; Leo Gershoy's, that historical narrative need not be subjective; and William Dray's, that the explanation of a historical action is the elucidation of the motives or reasons of the actor rather than exclusively the subsuming of the action under a law.

The first thesis, that historical narrative is chronicle plus "because" statements, is denied by some of the symposiasts on the ground that the nonchronicle part of historical narrative, rather than a web of "because" statements, is sometimes one of the following: the linkage of facts simply through their having in common "the property of being parts or aspects of that which is the central subject of the history" (Maurice Mandelbaum); the summing up and presenting as a whole (Albert Hofstadter); and the compound of "appreciation, valuation, and—our language is deficient here—the grasp of ultimate validities such as is striven for in art, religion, and philosophy" (Hofstadter).

The second thesis, that historical narrative need not be subjective, is supported by Sidney Hook, who writes, "Truths in history may be more difficult to obtain than in other areas of experience: they are not more relative."

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The third thesis, that the explanation of an action is the elucidation of the motives or reasons of the actor (rather than exclusively the subsuming of the action under a law), is initially defended by Dray in a restatement of the viewpoint of his *Laws and Explanation in History* (1957). According to his essay in the present volume, the aim of a rational explanation of an action "is not to show that the agent was the sort of man who does in fact always do the sort of thing he did in the circumstances he thought he was in" but rather "that the sort of thing he did made perfectly good sense from his own point of view." Objections to this position include that of Raziel Abelson, according to whom Dray's criterion "entails that every historical person is rational in the sense that, given the factual situation as he sees it, his actions are faultless"; that of Evelyn Barker, who writes that "even if there is a rationale for Nero's putting his mother to death, we would not call this a *rational* action"; and that of Donald C. Williams, who cites the negative correlation between what is rational in the sense of being the reasoning of the agent and what is rational in the sense of what satisfies the reasoning of the hearer or reader of the narrative.

The intelligence of the planning of this book and the effectiveness of the execution of the plan are manifest in the clear presentation of the views to be debated and the directness and precision of the confrontation between opposing hypotheses. The project represents a fruitful interdisciplinary conversation between reflective historians and philosophers experienced in writing intellectual history. One of the participants in the symposium, bemused by the references of the others to "working historians," asks if there is any other kind.

Washington, D. C. WILLIAM GERBER

CHRISTIANITY IN A REVOLUTIONARY AGE: A HISTORY OF CHRISTIANITY IN THE NINETEENTH AND TWENTIETH CENTURIES. Volume V, THE TWENTIETH CENTURY OUTSIDE EUROPE: THE AMERICAS, THE PACIFIC, ASIA, AND AFRICA; THE EMERGING WORLD CHRISTIAN COMMUNITY. By *Kenneth Scott Latourette.* (New York: Harper and Row. 1962. Pp. vi, 568. $8.50.)

IN the current volume a former President of the American Historical Association and one of the world's most honored historians concludes an impressive work of 2,750 pages. He has written church history on a truly global scale, a study of the Christian factor in broadly human history. Indeed, no one has tramped so completely over the bounds of place and time traditional among the scholastics of church history. Heinrich Hermelink wrote the only work to be thought of in comparison with Latourette's present endeavor, and it is second in all respects save the treatment of German Protestantism and of certain phases of European culture viewed from Germany.

Latourette's completed series has dealt first with the nineteenth century (I,

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Europe, background to 1815, and then the Roman Catholic phase; II, Europe, the Protestant and Eastern Churches; III, outside Europe—the Americas, the Pacific, Asia, and Africa); and secondly, with the twentieth century to date (IV, Europe, the Roman Catholic, Protestant, and Eastern Churches; and V, as above). This structure reveals a thesis and a contribution, namely, that in the nineteenth century Christianity in the European heartland of church and culture required twice the attention given to other continents, while in the twentieth, the ratio is one to one. Furthermore, as much attention now is given to Asia and Africa as to the Americas and Australia.

The earlier volumes have won high praise from historians in Britain and on the Continent as well as in North America; "indispensable" is the recurring phrase. They deal with the organizational and the spiritual life of the churches, with theological and related thought, with prominent leaders, with Christian activities in society and culture. There is no disposition to shrink from honest study of social and cultural influences upon Christianity, from aspects of weakness and decline. Coverage of books in the Western languages is remarkably extensive, including intellectual and social with general history. Volumes IV and V have the extra and emphatic merit of drawing together the most diverse materials, many of them only lightly worked by historians.

Latourette soundly organizes the chronological, geographical, and topical categories of his subject. The style is that of straightforward exposition. The work is a sound compendium seldom galvanized, seldom distorted, either by strong theses or by adventurous interpretations. Some philosopher-historians and theologians may and do complain that their truths do not direct Latourette's thought, that his catholicity lacks method, penetration, conviction. His supporters reply that his effort requires that "combination of objectivity and empathy" which cannot force the varieties of global experience into a system, either by exclusions or by homogenizing. Indeed, it would be difficult to surpass the author in blending three talents appropriate to his task: the understanding and the wide-ranging participation of one who is a committed Christian; the diligent and generally effective entry into the experience of other types of Christianity than his own; and the broad, free inquiry of the general historian at work in the complexities of the past 170 years.

Does anyone truly comprehend the depth and the entirety of the world's changes in these two centuries? The historian should be able to do so more adequately than many another. Yet a life given to study of the past tends so naturally to construct certain diagrams of the institutions and the minds of men that it is hard to adapt to the violent novelty and flux of recent decades. Despite remarkably wide acquaintance with living leaders in his chosen realms, Latourette seems, like so many students of history, to limp slightly behind the rushing years. The grip of understanding is inevitably less sure when it moves this side of 1918, and more obviously so when this side of 1940. Moreover, the old problem of the historian

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who tries to describe and in some measure to weigh the invisible and imponderable factors of spirit, no matter how faithfully he tries to sense them as interacting with observable facts and factors, economic and social, seems to be heightened as the technological era mass produces its statistics and its quantitative social science.

All the doubts and criticisms need to be assembled and considered, but Latourette's work will be greatly valued and long used, both for reading and for reference. He is informing a generation and is organizing materials toward the lasting record.

Union Theological Seminary M. Searle Bates

LITERATURBERICHTE ÜBER NEUERSCHEINUNGEN ZUR AUSSERDEUTSCHEN GESCHICHTE. Edited by *Walther Kienast.* [Historische Zeitschrift, Supplement Number 1.] (Munich: R. Oldenbourg. 1962. Pp. 779. DM 48.)

This volume is the first in a new series designed to report recent historical studies on countries other than Germany and intended to replace individual review articles that have hitherto appeared in the *Historische Zeitschrift* on such areas. These volumes are expected to be published every two years in such a way that each field will be reviewed about once in five years.

The present volume covers research that has been published since 1945, except in fields where the early postwar years have been the subject of previous review articles, and it contains the following eleven contributions: "Scandinavia" by Hermann Kellenbenz (from 1945 on); "Modern England" by Christopher Howard (1950–59); "Hungary" by Johann Bak (1945–60); "General and Medieval Poland" by Gotthold Rhode (1945–58); "Soviet Studies on the USSR" by Horst Jablonowski (1953–57); "Non-Soviet Studies on the USSR" by Klaus Meyer (1953–59); "Latin America" by Richard Konetzke (1945–59); "Southeast Asia" by E. Sarkisyanz (1945–58); "Japan" by Horst Hammitzsch (1945–56); "Medieval France" by Karl Ferdinand Werner (1952/54–1960); and "Medieval Italy" by Eugenio Dupré-Theseider (1945–58). Books are included regardless of previous review in the *Historische Zeitschrift,* and in many cases specific reference is made to such a review.

The treatment of the various areas is by no means uniform; the main underlying purpose is to provide German historians and indirectly also Western historians with a useful reference guide. Thus the most extensive treatment is given to countries, such as France and Italy, that have always been of special interest to German historiography. On the other hand, areas that have not been traditionally in the focus of German historical research receive less coverage. Accordingly, only works in West European languages are reviewed in the articles on Japan and Southeast Asia. While no similar language problem exists for Latin America, its history is relatively unknown in Germany, and the contributor takes this into consideration by providing a general survey.

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There are also methodological differences in the approach used by the various reviewers. For instance, the article on Scandinavia, which contains over six hundred references, more than any other contribution, is a highly compressed survey of Scandinavian historical scholarship. It lists not only books, but also numerous articles, and has almost the quality of a continuous narrative. Other reviews are more selective, draw more on the results of international scholarship, and, in addition to historiography, emphasize bibliography and other reference works. The article on Poland describes the impact of the changes in the political scene on historiography and provides a detailed statistical analysis of publishing trends in Poland.

Undoubtedly, many other such variations affecting emphasis, depth, and selection can be found. These differences are overshadowed, however, by the independent and sound scholarly judgment that permeates all contributions. In particular, partisanship seems to have been avoided, although controversial books are included. Moreover, the presentation is lucid and to the point, making it a useful and stimulating account for those who want to keep abreast with developments outside their own field. Altogether, this is a timely guide that provides a characterization of over three thousand clearly identified items. It is well edited and indexed and should be appreciated even more by the profession, once the complementary volumes have appeared.

Library of Congress ARNOLD H. PRICE

LES ÉTATS-UNIS DEVANT L'OPINION FRANÇAISE, 1815–1852. In two volumes. By *René Rémond*. Preface by *Pierre Renouvin*. [Cahiers de la Fondation Nationale des Sciences Politiques, Numbers 116 and 117.] (Paris: Librairie Armand Colin. 1962. Pp. viii, 445; 449–967.)

THIS comprehensive study of the evolution of French opinion of the United States from 1815 to 1852 by René Rémond, director of studies and research at the *Fondation Nationale des Sciences Politiques* and author of several books on French Catholicism and the Right in France since 1815, continues the investigation initiated by Gilbert Chinard and carried on by Durand Echeverria and others of the French image of American society to 1815. Since there are similar studies for the Second Empire, the subject has now been thoroughly explored down to 1870, except for studies in detail, new interpretations, or new methods of procedure.

Rémond's massive doctoral dissertation is notable for the originality of its method, in the view of Pierre Renouvin, and for its wide scope, exhaustive treatment, clarity of organization, vivid style, and significant conclusions. The author, who is greatly concerned with method, is convinced that studies of public opinion must be based upon a deep knowledge of the historical background. Therefore in the first volume he makes a detailed analysis of Franco-American relations in the first half of the nineteenth century and of the concepts of the United States derived from French political refugees, utopian socialists, emigrants, churchmen,

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Freemasons, Louisiana settlers, men of letters, merchants, and diplomats. These relations became closer as transatlantic travel improved, especially in the regions around Paris, Le Havre, Bordeaux, Lyon, and Strasbourg. Since Rémond believes that the fundamental problem is the relationship between information and public opinion, he appraises the influence upon public opinion of French and British publications about America, the French literary tradition, and American literature. He contends that published works are of paramount importance for his subject and discounts the value of archival materials, particularly diplomatic papers, which were read by a very limited group. He has explored the French archives, but has not consulted American archives for such documents as reports from American consuls in France. He has read all the published works on his subject, which he lists chronologically in an extensive and useful bibliography, and has read widely in periodicals and sampled newspaper opinion as well.

From these sources, in his second volume Rémond endeavors to re-create the evanescent French "collective representation" of the United States in his period (a term he prefers to "myth," "image," or "mirage," since he is equally concerned with thought and judgments), to identify its themes, and to trace the changing and diverse attitudes of French political groups toward the United States. The general public was grossly misinformed as to American geography and had only a shadowy conception of an American. Under the Restoration, French political affiliations determined public opinion of America. The Liberals, and some Ultra-Royalists, who condoned the American Revolution as moderate, continued until 1832 to believe in the eighteenth-century mirage of America. Because of classical influences, the literary tradition, and admiration for Franklin, they pictured the United States as the land of the "noble savage," virtuous farmers, and sober Quakers, who lived in a happy state of nature and freedom under a wise and beneficent government, which served as a model to all mankind. Lafayette's triumphal visit to the United States in 1824–1825 and the Revolution of 1830 strengthened this image, which was held by the majority of Frenchmen. Under the July Monarchy the mirage disappeared, because of more precise information, the critical attitude of British observers, the acute controversy in 1834–1835 over the American claims for damages to shipping during the Napoleonic Wars, and the advent to power of the Liberals, who no longer needed to cite the American experience and feared its use by the opposition. The American government was now represented as demagogic and American society as materialistic, increasingly industrial, and harsh toward Negroes and Indians. During the Revolution of 1848 the conservative and liberal Right pointed to the American example in support of its struggle against radical democracy and socialism. It was not until the Second Empire that the model of the United States was used once more as a weapon against an authoritarian regime. In expressing their opinions of the United States, the French became more conscious of their own identity and of the originality of their aristocratic, Latin, Catholic, and agrarian culture. Rémond concludes that greater knowledge of another people does not necessarily bring greater

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comprehension or friendship. The significance of this major work for further research in public opinion and for a deeper understanding of international relations is apparent.

State University College, New Paltz, New York EVELYN M. ACOMB

VSEMIRNAIA ISTORIIA [Universal History]. Volume IX. Edited by *L. I. Zubok et al.* (Moscow: State Publishing House for Socio-Economic Literature. 1962. Pp. 748.)

BEGINNING with the relative stabilization of capitalism from 1924 to 1929, this volume proceeds to the Great Depression and the first Five-Year Plan, the partial economic recovery in the mid-1930's, the rise of fascist aggression, and the coming of the Second World War. Although frankly Marxist in tone, it presents quantities of factual material that one would find in a standard Western work. There is, however, special emphasis on the rivalries of capitalist groupings, with American financiers playing an increasingly active part. The volume also stresses the stubborn opposition of workers and peasants everywhere to exploitation, with the Communists usually in the forefront. Moreover, the struggle of the colonial peoples against imperialist domination is another persistent theme.

The ending of the Ruhr crisis and the introduction of the Dawes plan ushered in a recovery of European capitalism, accompanied by increasing penetration by American finance. But even these golden years saw sharp class conflict in Britain, France, and Japan, and farm crises and great strikes in the United States. In the small states of Eastern Europe, where prosperity was scarcely felt, workers and peasants reacted against their misery by strikes, riots, and revolutionary outbreaks. Only in the USSR was there steady economic progress.

This was also an era of colonial revolts. The Chinese revolution made spectacular progress until Chiang Kai-shek and the Nationalists turned on the Communists and sought to drown the revolution in the blood of peasants and workers. Other powerful insurrectionary movements appeared in India, Egypt, Indonesia, Syria, and Morocco, only to be crushed by massive use of troops. In Latin America there were increasing signs of opposition to imperialism.

In the field of international relations, the authors regard all the major powers as sworn enemies of the USSR, although the rivalries of British and American capital and British and Italian efforts against French hegemony in Europe prevented the crusade against Communism that some British leaders eagerly sought. The USSR earnestly worked for disarmament and made nonaggression pacts with its neighbors.

Like many Western scholars, the Soviet historians feel that the depression of 1929 pushed the world along the road to disaster, with the rise of German fascism and Japanese aggression in China as major symptoms, while economic ruin was widespread. Only in the USSR were there rapid progress and constructive achievement, in spite of the serious errors of Stalin. In other countries the

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Communists gave leadership to the masses in their struggle against fascism. Unfortunately, the Comintern Congress in 1928 branded all socialists as "social-fascists," to be combated without mercy, and also held that the national bourgeoisie in colonial lands were reactionary. At the eleventh hour the German Communists sought a common front with the Social Democrats against the Nazi menace, but the latter rejected the proposal.

While critical of Stalin's growing tyranny (which, the authors state, led many Communists to wish to remove him from power) and the totally unjustified purges of 1936–1938, this volume strongly supports Soviet efforts of the 1930's to promote collective security and the popular front. Their failure was caused, not by Communist errors, but by capitalist fear of the increasing strength of the USSR and a growing readiness to appease aggressors in hopes that they would turn on Communist Russia. Hence the betrayals: in Ethiopia, in Spain, in Czechoslovakia. Only Soviet backing and aid, the authors insist, kept China from falling to Japan, as Britain, France, and the United States made concessions to the aggressors and sold them vital supplies. Finally, as Hitler prepared to attack Poland, Britain and France sought to embroil the USSR in war against Germany without committing themselves to action. At this point the Soviet leaders, realizing that collective security was dead, signed a treaty with Germany, thereby winning a period of peace for their country. The inevitable conflict thus began with the Germans moving, not against the Soviets, but against the West.

The last section of the text contains an inclusive and clear summary of the general achievements of science and technology from 1900 to 1939, followed by a consideration of art and literature in the USSR and in the capitalist world. While the authors of this section strongly favor writers and artists of the Left, they also see merit in non-Marxists who displayed a social approach and progressive tendencies, such as John Galsworthy, James Joyce, Hemingway, Thomas Mann, García Lorca, Lu Sin, and Rabindranath Tagore. Their creative efforts, they feel, are helping to prepare mankind for a better social order.

Like its predecessors, this volume is well illustrated and provided with fine maps, tables, and a full chronology. There is an extensive bibliography of sources and monographs in at least twenty-five languages.

Duke University JOHN SHELTON CURTISS

Ancient and Medieval

ÉTUDES ARCHÉOLOGIQUES: RECUEIL DE TRAVAUX. By *Robert-P. Charles et al.* Published under the direction of *Paul Courbin.* [École Pratique des Hautes Études, VI[e] Section. Centre de recherches historiques. Archéologie et civilisation, Volume I.] (Paris: S.E.V.P.E.N. 1963. Pp. 230.)

FOURTEEN French scholars here issue, under four headings, a plea for mod-

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ernizing archaeological techniques and applying the results to economic history, de-emphasizing the history of art.

In "Preliminary Surveys," J. Vercoutter laments the parsimony that will let the Assuan Dam inundate one hundred sites in Sudanese Nubia, and R. Chevallier describes the efficiency of air photography, permitting, for example, a reconnaissance of Metaponto (five hundred hectares) in only twenty hours.

André Leroi-Gourhan deplores the losses to the record due to amateur and clandestine digs, while bona fide ones have inadequate budgets; he urges archaeologists to invite specialist collaboration in "Excavation." P. Courbin expatiates on the accuracy of his own stratigraphy at Argos and excoriates a previous excavation where Byzantine remains were mistaken for Hellenistic.

Within "Interpretation," A. France-Lenord lists new techniques—X ray, spectroscope, bombarding ancient coins with neutrons—and proposes a central laboratory for archaeological analysis; Arlette Leroi-Gourhan describes the technique and results of pollen analysis; R.-P. Charles speaks for collaboration between archaeologist and anthropologist; J.-Cl. Gardin argues for a new code to describe finds objectively, thus enabling the archaeologist to use the energy spared from compiling written inventories to draw essential conclusions.

Covering "Economic and Social History," E. Will displays extremely limited enthusiasm for the vogue of ancient economic history, where, he thinks, too sweeping conclusions are too often drawn from inadequate evidence, though more could be done by a new analysis of pottery, and by studying sites from an economic viewpoint, as Roebuck has done for Ionia. J. Deshayes compiles distribution maps of Bronze Age tools to place the apogee of Crete in the sixteenth to the fifteenth century B.C., of the Argolid in the fifteenth to the fourteenth. G. De Rider records useful techniques in numismatics (neutron bombardment, die sequences), and shows how mine marks, countermarks, and changes of standard contribute to economic history. Fernande Ducat shows how careful stratigraphy at Byzantine levels in Argos illuminates an obscure period. G. Vallet and F. Villard write on the contribution to economic history of the study of pottery: vases as containers or as works of art, the different significance of pottery as import and as export, the false antithesis between high quality and mass production (Athenian potters were capable of both), the use of amphorae to establish trade routes. Finally, A. Varagnac ("Archéocivilisation") suggests how folklorists can use archaeology to determine which modern folkways are prehistoric survivals.

This book projects abroad a distorted image of French archaeology, which is not as backward as Courbin implies. Chapouthier at Mallia, Schaeffer at Enkomi, Bloch at Bolsena, and Courbin's particular bête noire, R. Martin, on Thasos, have all done good work, useful to economic historians. *Odium archaeologicum,* a deadly heritage from the philologists, infects many of the essays, especially Courbin's. In their enthusiasm for economic history some of the contributors (Will, Vallet, and Villard excepted) throw out the baby with the bath, neglecting archaeology's importance for the historian of art or politics. The folklorist

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generalizes from insufficient knowledge; the water mill is older than he thinks (at least Trajanic), and examples, apparently unknown to him, exist of cavalier treatment of earlier Bronze Age tumulus burials by later ones, as at Enkomi. There is nothing on electrical resistivity surveys (Lerici in Etruria), nor on exploiting the magnetizing of fired clay, nor on carbon fourteen, nor on the use of bone marrow to aid the ethnographers by determining blood groups. Fortunately the essays on air photography, metallurgical techniques (France-Lenord), numismatics, and general ceramics strike a healthy, positive note and do much to counteract the pervasive air of querulousness.

University of Wisconsin PAUL MACKENDRICK

THE SUMERIANS: THEIR HISTORY, CULTURE, AND CHARACTER. By *Samuel Noah Kramer*. (Chicago: University of Chicago Press. 1963. Pp. xiv, 355. $7.95.)

"HE who knows, why should he keep it hidden?" is a Sumerian proverb that could serve as an apt motto for the title page of this long-awaited synthesis in English of what is known or—and this needs emphasis—can be reasonably conjectured about man's first advanced civilization. A steady stream of publications during the past twenty-five years has established Kramer's international reputation as one who "knows" the highly specialized field of Sumerology, and the present lucidly written volume is the product of his belief, well founded, it turns out, in the humanistic value of the Sumerian literary remains.

Kramer's method consists largely in centering the discussion on some given document, analyzing it in detail, and translating it in whole or in part. Since most Sumerian texts have a relevancy to several facets of the civilization, numerous cross references are required, and there are many instances where pertinent material presented elsewhere in the volume is overlooked.

After an introductory chapter on Mesopotamian archaeology and decipherment that is a model of concise clarity, the chapters on "History" and "Society" present the longest and most detailed survey available of Sumerian political, social, economic, legal, and technological achievements to the time of Hammurabi (about 1750 B. C.), when the Sumerians "ceased to exist as a political, ethnic, and linguistic entity." Kramer stresses the results of recent investigations (for example, the prevalence of privately owned land in early Sumer) and appends a translation of thirty-two pages of pertinent historical and legal documents.

Perhaps the strongest of the book's eight chapters are the two on religion and literature. Of particular interest here are the "first precious examples" of the elegiac genre and the evidence they contain for judgment after death. Strong, also, is the chapter on the Sumerian school, the remarkable secular institution on which much of the vitality of the civilization apparently rested.

In a chapter on "Character," Kramer "hesitatingly" singles out love, hate, fear, rivalry, and superiority as the major motivating forces behind Sumerian behavior.

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Most convincing and detailed is his treatment of love, yet he slights the moral manifestations of altruistic love.

In summing up "The Legacy of Sumer," Kramer gives a valuable account of Sumer's contacts with its neighbors, but the remainder of this chapter is weakened by a tendency to confuse borrowings and parallels (for example, the city-state, epic literature, moral concepts, written law). Although the volume contains very few footnotes, references are frequently cited in the text, and there is a valuable select bibliography. There are numerous plates and figures and a full index, but no map.

Tulane University NELS BAILKEY

PEOPLE OF ANCIENT ASSYRIA: THEIR INSCRIPTIONS AND CORRESPONDENCE. By *Jørgen Læssøe*. Translated from the Danish by *F. S. Leigh-Browne*. (New York: Barnes and Noble. 1963. Pp. xx, 169. $6.50.)

AT the outset the author takes the position of apologist for the Assyrians and states his intention to "rehabilitate" their gory popular reputation, gained largely from the Old Testament and the royal sculptures and inscriptions, by using as source materials the thousands of private documents excavated in the last generation or so. This is a novel approach, and one that might be expected to provide much supplementary, and sometimes corrective, detail to elucidate the picture given in official sources. It does not seem, however, that this intention has been fulfilled, due chiefly to the uneven coverage of the subject.

The author's main interest is clearly the early period of Assyrian history, particularly the reigns of Shamshi-Adad I and his immediate successors (roughly from the mid-eighteenth century B.C. to the early seventeenth century), and his chief source is the royal correspondence from the city of Mari on the Euphrates, one of the western outposts of the early Assyrian Empire. After a brief general survey of Mesopotamian history, with special stress on the outside peoples who entered the land from time to time, Læssøe turns to Assyria and describes in some detail the relationships between Shamshi-Adad and his sons, Jasmah-Adad, viceroy in Mari, and Ishmê-Dagan, viceroy in the East, and that between the two brothers after the disappearance of Shamshi-Adad from the scene. The "Hurrian interlude" deals briefly with the gradual incursion of this still-enigmatic people into the Assyrian area and their rise to political and military power between about 1450 to 1350 B.C. Here I must take exception to Læssøe's ideas that "it is not unlikely that the Assyrians . . . took over important elements from the art of the Hurrians," or that the inclusion of names of Hurrian deities in the Assyrian pantheon is evidence that "Hurrian ideas had profoundly permeated Assyrian society." Some influence is possible, but the evidence scarcely supports such strong statements.

The discussion of the Assyrian Empire is strangely brief, consisting largely of a description of the capital city of Nimrud, with translation of some new imperial documents found there. This may be considered a salutary corrective to the usual

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picture of the Empire as a series of military campaigns, yet seems in its turn to give a rather unbalanced picture of the period. "Assyrians and Hurrians in the Zagros" is concerned with the fortunes of the eastern part of the Assyrian Empire, especially as revealed by recent excavations at the site of Shemshâra, the account of which is a mixture of historical information and popular travelogue. The final pages on "Assyrian Historiography" endeavor to show the relevance of the material gained from private letters to the historical picture of the official inscriptions.

There is much useful material here, especially the extensive translations of the letters, most of which have not before been readily available to the general historian. The unbalanced nature of the treatment does not, however, give the general reader a good, coherent picture of the people of ancient Assyria.

Yale University ANN PERKINS

THE CONFLICT BETWEEN PAGANISM AND CHRISTIANITY IN THE FOURTH CENTURY. Edited by *Arnaldo Momigliano.* [Oxford-Warburg Studies.] (New York: Oxford University Press. 1963. Pp. 222. $5.60.)

THESE eight lectures, delivered by specialists at the Warburg Institute four years ago, are loosely connected by the theme of the series and by the introduction subsequently provided by one of the lecturers as editor. Momigliano reviews concisely the problems of the decline of the Roman Empire and proposes as the central feature of the decline in the fourth century "the emergence of the Church as an organization competing with the state." The lecturers, however, do not offer evidence for an institutional struggle between Church and state. The four who deal with the relations of the Empire and Christianity are concerned instead with the competition of Christians and pagans, of *novi homines* and old aristocracy for the control of the state. Thus the failure of Constantine early in the century to create a Christian dynasty (analyzed by Joseph Vogt in the second lecture) was balanced by the success of Gratian and Theodosius, under the episcopal admonitions of Ambrose, in overcoming the aristocratic revival of traditional Roman piety toward the century's end (as recounted by H. Bloch in the last lecture). The outcome was facilitated not only in the new capital in the east but even in old Rome by the rise of a new nobility from the middle and lower classes, fostered by the emperors from the time of Diocletian and receptive to Christianity. This aspect of social mobility is described in A. H. M. Jones's clear and critical examination of "The Social Background of the Struggle." Another example of the influence of the Empire, though not of imperial policy, upon the expansion of Christianity is provided by E. A. Thompson's careful and persuasive weighing of probabilities in the light of the sparse evidence on "Christianity and the Northern Barbarians." He concludes that the Germans (with one exception to prove the rule) were converted after, and not long after, settling on Roman soil. He does not point out

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the ironic significance for both Church and Empire of the fact that the conversion was owing to the Arian heresy.

The other four essays largely eschew political and ecclesiastical problems and attend rather to cultural elements that were inherited, not without conflict, by Christian Europe from antique paganism. Thus, A. A. Bart attributes "the magical arts" not to the survivals of primitive religion but to the disintegration of paganism, and Henri Marrou and Pierre Courcelle explore the contacts of Platonism and Christianity. Courcelle deduces the Platonist anti-Christian arguments from a textual study of Christian apologetics, while Marrou, from the writings of Synesius, bishop of Ptolemais, explains the compatibility of Alexandrian Neoplatonism with Christian doctrine. Synesius was a Greek representative of that *urbanitas* which the pagan aristocrats of Rome (as Bloch shows) sought to preserve against Ambrose, however Platonist he too was. Of all the essays comparing pagan and Christian culture, perhaps the most fascinating for the historian of any period is Momigliano's perceptive account of the invention of documented ecclesiastical history and saintly biography. Eusebius used pagan chronology, but otherwise paid as little attention to the classical tradition of secular history as Ammianus, writing in that tradition, did to Christian historiography. This lecture is a prime example of the attractiveness and scholarly importance of the volume for the general historical reader as well as for the specialist in the fourth century.

Woodrow Wilson National Fellowship Foundation R. F. Arragon

THE AGE OF THE VIKINGS. By *P. H. Sawyer*. (New York: St Martin's Press. 1962. Pp. vi, 254. $7.00.)

This book will come as a surprise to the many historians who believe that the Viking raids on England and the Continent from the eighth to the eleventh century meant nothing but war and misery for the inhabitants, pillage of churches and monasteries, burning of cities—in short, utter destruction wherever the Vikings appeared. Sawyer's thesis is that since most of the contemporary written evidence comes from ecclesiastical sources, chronicles, and hagiographic accounts, and since the churchmen suffered most from the Viking raids because of the rich and easily obtainable loot they offered, their descriptions must be overdone, their figures exaggerated, and their viewpoint biased. Through the centuries these accounts have, however, been repeated again and again, and their views have been the accepted ones, even in today's textbooks. Instead, Sawyer proposes to use all other kinds of available evidence to check the bias of the written sources. He reports the most recent results from archaeology, the study of place names and numismatics, and he tries to see the written sources in the light of these auxiliary sciences.

His book consists of three parts. The first five chapters deal with all the available material from the Viking period, both written sources and archaeological material, including ships and treasure. The chapter on treasure contains much

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interesting statistical material concerning the thousands of Arabic coins, the so-called Kufic coins, found especially in Sweden and Russia, and is based on unpublished work by the late Swedish scholar, Sture Bolin. The second part consists of Chapters VI–VIII. Sawyer here examines some of the basic assumptions that normally are made about the Viking period. He deals with the raids, settlements, towns, and trade. His conclusions represent a scaling down of the picture drawn by the churchmen. One of the most controversial questions in the period is the size of the Viking armies. The author argues convincingly that the largest army in the ninth century probably was under one thousand men and that most bands consisted of three or four hundred men. He thinks that the armies at the end of the tenth and the beginning of the eleventh century were also of similar scale. This seems more questionable since at this point they were much better organized than earlier. The Danish and Norwegian kings themselves directed the wars against England, while in the ninth century they disclaimed responsibility for the raids. The evidence of four well-known, large, fortified camps in Denmark points to larger armies, although Sawyer argues that these camps were built after and not before the conquest of England. The third part of the book (Chapter IX) treats the fundamental causes for the Viking attacks, which Sawyer attributes to the search for new land.

The author handles his material with all its linguistic problems very well. He does not deal with the very recent discoveries of Helge Ingstad at Lance-aux-Meadows, which give new certainty to the old claims that the Vikings discovered America. There is no new research in this book, but many of the interpretations are new, and one would not be surprised to see them adopted in future textbooks.

Baltimore, Maryland JENNY JOCHENS

CELT AND SAXON: STUDIES IN THE EARLY BRITISH BORDER. By *Kenneth Jackson et al.* (New York: Cambridge University Press. 1963. Pp. viii, 364. $9.50.)

Celt and Saxon, edited by Mrs. Nora K. Chadwick, is the third in a valuable series of cooperative works by Cambridge historians on the obscure period in British history between the Roman withdrawal and the advent of the Anglo-Saxon kingdoms. Like the first two books in the series, *Studies in Early British History* (1954; reprinted in 1959) and *Studies in the Early British Church* (1958), *Celt and Saxon* stresses the Celtic contribution to the development of medieval England. For a number of reasons, linguistic among others, Celtic Britain has long suffered from scholarly neglect. Consequently the studies of Chadwick and her collaborators assume the nature of pioneer works. They cast welcome light into numerous dark corners of early Britain, clear up many old problems (as well as raising some new ones), and banish more than one hallowed myth. Chadwick believes that the time for a coherent history of Celtic Britain has not yet arrived—that much ground must first be cleared. Above all, the historical

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source materials must be criticized, interpreted, and placed in their proper context.

The present book addresses itself to precisely that problem. In Chadwick's words, "no attempt has been made to trace the actual history of the overlap of the British and the Anglo-Saxon peoples on our Borders. Our aim has rather been to examine the sources of such history and to seek to establish their relative value by inquiry into their origin, the conditions in which they came into being, the purpose for which they were recorded, and the bias of their sympathies."

In the studies that follow, Chadwick and her distinguished collaborators examine a series of crucial sources of the period. Kenneth Jackson reconstructs the lost northern sources used by Nennius in his *Historia Brittonum* and suggests that Nennius depended in part on a lost northern history written in the later eighth century. Peter Hunter Blair analyzes the valuable early annals that were copied into the twelfth-century *Historia Regum* of Symeon of Durham. Bertram Colgrave studies the *Life of St. Gregory the Great* by an anonymous monk of Whitby, a contemporary or near contemporary of Bede, which provides important supplementary information on the conversion of Northumbria. Bruce Dickins contributes a short note on *Dewi Sant* (the Welsh form of St. David) as it occurs in western border sources and place names. H. M. and Mrs. Joan Taylor examine and compare church architecture and sculpture on the northern and western border where Celtic and Anglo-Saxon artistic traditions met. Christopher Brooke investigates the documents of two monastic houses of the southern Welsh border, Gloucester Abbey and Llancarfan, which provide intriguing material on the history of the area in the late eleventh century and earlier. And Chadwick herself provides three illuminating studies on Bede, the sources for the conversion of Northumbria, and the sources for the Battle of Chester.

These essays are unavoidably heterogeneous; the editor wisely allowed her colleagues a loose rein, and as a consequence the studies range rather widely within the broad theme of the book. But what they lose in uniformity they gain in originality and scholarly precision. These are the works of skilled specialists working in fields that they know and love. Every one of the essays is more than competent; several are brilliant. Their total effect is to clarify very considerably our knowledge of the sources of Anglo-Celtic history.

University of California, Santa Barbara C. Warren Hollister

LIBER GROSSUS ANTIQUUS COMUNIS REGII ("LIBER PAX CONSTANTIAE"). In 6 volumes. By *Francesco Saverio Gatta*. [Biblioteca della Deputazione di Storia Patria dell'Emilia e della Romagna, Sezione di Modena; Biblioteca della Deputazione di Storia Patria per le Antiche Provincie Modenesi, Sezione di Reggio-Emilia; Deputazione di Storia Patria per le Antiche Provincie Modenesi, Sezione di Reggio-Emilia.] (Reggio: [the Biblioteca.] 1944; 1950;

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1960; 1960; 1962; 1962. Pp. xxxi, 314; xv, 343; xi, 338; xv, 316; xvi, 274; xx, 254.)

THIS massive work represents a diplomatic edition of one of the prime sources for the history of medieval Reggio, the *Liber Grossus Antiquus* or, as it is often called, the *"Liber Pax Constantiae,"* because its first document is the text of the Peace of Constance of 1183. The original codex that has been reproduced in these volumes contains 691 documents. The earliest document is a charter granted by Emperor Otto I dating from April 20, 962; the latest is dated November 3, 1352. Most of the documents, however, are concentrated between 1180 and 1300; only sixteen predate 1180, and only thirty-four postdate 1300. Many of the documents have been printed in whole or in part in other source collections, but this is the first publication of the entire codex.

As the editor explains in the preface to the first volume of this work, the *Liber Grossus Antiquus* represents a selection of documents first undertaken in 1270 at the instigation of the Podesta Oddo degli Oddi, who ordered various notaries to transcribe material from a register maintained by the commune of Reggio since 1228. The exact reason for this undertaking is not clear, but Gatta suggests that the confused condition of the communal register may have dictated a greater systemization of communal records, especially those crucial to the city's affairs. Apparently a nucleus of documents, comprising about one-half of the total collection, was made initially, and then documents were added to the codex in suceeding years. The documents are not inserted in the codex in chronological order, suggesting that some other rationale governed the selection and ordering of the documents. The criteria followed are not, however, immediately obvious, so that the *Liber Grossus Antiquus* seems to lack any rational order. Gatta offers no adequate explanation for its organization, although one suspects that this question might be cleared up by a thorough analysis of the content of the documents.

In editing the *Liber Grossus Antiquus,* Gatta has chosen to reproduce the documents as they stand in the codex. With the exception of certain important documents, for which other copies exist and have been edited, he has made no attempt to discover the original or older versions of the documents contained in the *Liber Grossus Antiquus* in order to ascertain the variants in the material he is editing. Although he speaks in his preface of certain difficulties in transcribing his text, there is throughout a minimum of apparatus to indicate or explain troublesome readings. Bibliographical citations are confined to noting those collections in which various documents have previously been printed in whole or in part. Gatta occasionally provides a brief marginal note explaining a passage in a document, but in general he supplies a minimum of guidance in clarifying the numerous difficult points in the text. He does give at the head of each document a summary of its content in Italian, a device helpful in identifying the document.

The methods followed in editing the work, combined with its very nature, will create certain difficulties for potential users. The documents are not in chrono-

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logical order; thus, one might have to employ all six volumes to discover materials pertaining to any particular period. Gatta has attempted to offset this difficulty by providing a chronological listing of the documents and a brief description of each item at the end of each volume, but a single listing of all the documents in chronological order would be more useful. The lack of explanatory notes will require each user to do his own identification of individuals named and events referred to in the numerous documents. Some may not be content to use the documents in this collection since all are copies; these purists will have to search out the originals themselves, for this edition offers no guidance on this matter except in a few cases. No index exists, although the editor promises one.

If one can master the material collected here, he may indeed gain "la visione panoramica della storia di Reggio." It is impossible in a brief review to give an adequate summary of the materials contained in the eighteen hundred pages of text. There are numerous types of documents: treaties, statutes, charters, judicial acts, letters, privileges, inventories of goods and properties, oaths of fidelity, and transfers of property. A cursory examination of the contents of the documents suggests that the collection might be most valuable for reconstructing the relations of Reggio with neighboring communes, for tracing the submission of the feudality to the commune, and for ascertaining the financial policy of the commune. But it is obvious that embedded in these documents is a rich store of material relative to every aspect of life in a typical Italian commune during a vital era in its history. The publication of this material will serve medievalists well.

Michigan State University RICHARD E. SULLIVAN

LA COUR PONTIFICALE D'AVIGNON (1309–1376): ÉTUDE D'UNE SOCIÉTÉ. By *Bernard Guillemain*. [Bibliothèque des Écoles françaises d'Athènes et de Rome, Number 201.] (Paris: Éditions E. de Boccard. 1962. Pp. 807.)

ALTHOUGH this volume is entitled *The Pontifical Court of Avignon,* its subtitle, *Study of a Society,* more accurately conveys the author's purpose. It is a detailed historico-sociological analysis of the unique society that was established at Avignon during the residence of the popes. Such an analysis, of course, requires a consideration of the *Curia* proper, its personnel and its functioning, but it includes far more. For the author's definition of society embraces not only the entire administrative and service personnel, but also that large number of persons known as *cortisani,* who, unlike the curialists, were without determined function, and the original citizens of Avignon. The archival and documentary materials bearing on the Avignon papacy are abundant and have elicited considerable interest in recent years. But they have not hitherto been exploited for the same purposes as those to which Guillemain has addressed himself. In short, this is a study essentially different from the classic work of Mollat or the numerous monographs on papal administration, curial departments, individual popes and cardinals.

The significance of this study is both special and general. It is special because

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Avignon, a city not formerly associated with the papal administration, was thus associated for the better part of a century. Indeed, the papal court, normally peripatetic, was stationary at Avignon for an exceptionally long time. As a consequence, the city was profoundly altered in character and almost completely transformed as to population. Avignon does not, therefore, constitute a typical example of urban development. But though unique, this society indirectly affected the entire Christian world. For it was in the midst of this cosmopolitan population, composed predominantly of immigrants who far outnumbered the original citizens, that the popes, the cardinals, and a host of minor functionaries lived and worked. And it is the author's contention that before any adequate generalizations can be made regarding such matters as the strength and weakness of the Avignon papacy, the role of relatives and friends of popes and cardinals, the influence of the French monarchy, or Avignon's place in the development of Renaissance culture, it is necessary to have a more accurate picture of Avignon society than has hitherto emerged. He is well equipped to perform this task. A former member of the *École Française de Rome,* he was able during his stay there to examine the relevant manuscripts in the Vatican archives, and he is thoroughly familiar with published sources and recent monographic literature. To the latter he has himself contributed significantly.

The procedure has been to exploit all possible sources of information concerning individual persons resident at Avignon. In the process certain things noted before, for example, the predominance of southern Frenchmen and the preference of the popes for those of common regional background, are here more carefully documented. But other conclusions emerge. Special skills required more distant recruiting. In the curial bureaus, for example, the French element was less prominent (59 per cent) and the Italian community still considerable (31 per cent). Musicians came largely from the north. The *cortisani,* it appears, were widely divergent as to origin, but principally came from a region bounded by the North Sea and the Channel, the Rhine, a line running from Toulouse to Rouen, and the Po and Tiber to the south. This suggests an attraction to Avignon based on its location on the great north-south avenue of communication of Western Europe.

These are but a few of the significant conclusions suggested by Guillemain. He also feels that, despite the multiplication of distinctions according to occupation, the ancient patriarchal character of the papal "family" was preserved. Moreover, the meticulous care with which all regular and special liturgical functions were performed suggests a greater solicitude for the religious character of their high office than has sometimes been attributed to the Avignon popes. Similarly, although the presence of Petrarch, Italian painters, and Flemish musicians suggests that new horizons were appearing, the cultural interests of the popes seem to have remained within the tradition of religious learning and liturgical art. If certain of the cardinals evinced some concern for the new more secular studies then engaging the attention of princes, their patronage does not seem to have been particularly enlightened.

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Guillemain has illustrated his findings graphically with the aid of 11 maps and some 150 statistical charts and tables. Moreover, the information presented includes not only population figures, but analyses of such matters as place of origin, occupation, and education. The bibliography is exhaustive, the index detailed. The whole is a work of profound scholarship, in the best tradition of the series to which it belongs. It is likely to remain for some time the definitive treatment of that unique society which was papal Avignon.

New York University MARSHALL W. BALDWIN

HISTOIRE DES INSTITUTIONS FRANÇAISES AU MOYEN ÂGE. Published under the direction of *Ferdinand Lot* and *Robert Fawtier*. Volume III, INSTITUTIONS ECCLÉSIASTIQUES. By *Jean-François Lemarignier et al.* (Paris: Presses Universitaires de France. 1962. Pp. viii, 502. 30 fr.)

IT is easy to see why the editors had some hesitation in devoting a volume of the history of French institutions to the history of the Church in France. It was evident that scholars writing on this subject could escape from generalizations applicable to the Church in any country only at the risk of becoming somewhat anecdotal. This danger has not been entirely avoided, especially in the section on the French Church in the fourteenth and fifteenth centuries. Moreover, in trying to discuss both the internal history of the Church and the history of its relations with the secular power, the authors found themselves running short of space and unable to do full justice to either topic. It is rather difficult, for example, to add much to our understanding of the conflict between Philip the Fair and Boniface VIII in twelve pages.

On the whole, the quality of the book declines as it advances in time, just as the quality of the Church declined after its heroic age. Lemarignier's section on the post-Carolingian Church, the feudal Church, and the Church in the period of the Gregorian reform is excellent. He rather minimizes the weaknesses of the feudal Church—weaknesses that his other writings have shown he knows very well—but his chapter on the impact of the reform movement is first rate. Gaudemet's section on the Church from 1150 to 1314 is a rather dry compilation of well-known facts and must have been written in haste. The author leaves readers completely confused about the difference between the Saladin tithe and later tenths collected from the clergy alone; he suggests that only Jews lent money at interest in the thirteenth century; he calls Henry III's brother "duke" of Cornwall; and he fails to give any real explanation of the quarrel over the mendicant orders in the University of Paris.

Mollat's section on the late medieval Church is very uneven. As was said above, it is full of anecdotes, and the author, like the late Professor Coulton, has a tendency to jump about in space and time in selecting his material. He is at his best in discussing Church-state relationships, although his line of thought is not easy to follow when he gets to the period of the Great Schism. At least he

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makes it clear why most decent men of the early fifteenth century thought that drastic reforms in the Church were necessary.

The book will be useful as a summary of present knowledge and as a guide to more detailed studies, but it illustrates once more the difficulty of writing a history of institutions that covers more than a century or two. We all know that institutions reflect the society in which they exist fully as much as they shape it. But when one has to describe institutions developing over a period of five or six centuries there is apt to be no space in which to describe the society as well. This means that the author cannot give his interpretation of the pressures that affected institutional development, and this means in turn that his exposition of institutional development will seem incomplete and occasionally incomprehensible. The learned authors of this volume could not escape this difficulty; this is why, with all their erudition, they did not produce an entirely satisfactory work.

Princeton University JOSEPH R. STRAYER

ASPETTI DELLA VITA ECONOMICA MEDIEVALE (STUDI NELL-'ARCHIVIO DATINI DI PRATO). Volume I. By *Federigo Melis*. (Siena: Monte dei Paschi di Siena; distrib. by Casa Editrice Leo S. Olschki, Florence. 1962. Pp. xxiv, 729. $45.00.)

THIS massive, elegant, and very expensive volume is dedicated to the career of Francesco Datini (1335–1410), the Tuscan merchant whose business papers provide the most detailed record of medieval business activity. For nearly a century, students have been working in this vast collection in Prato comprising more than 150,000 items, and their research has substantially enlarged our knowledge of European mercantile activity in the fourteenth century. Melis is the first scholar to explore systematically the entire Datini archive; this volume is an impressive monument to his industry.

After a brief survey of the archive's history and a section devoted to Datini's life and character, the author develops his main themes: the internal organization of the Datini mercantile and banking companies, the accounting methods utilized by these firms, and the operations of the cloth manufacturing industry. These problems are described in great detail and extensively documented. Selections from hundreds of business documents in the Datini archive have been incorporated into the text and the footnotes, and these constitute a valuable collection of source materials for the student of medieval economic history. Part of the documentation consists of graphs, charts, and statistical tables, which both embody and illustrate the author's conclusions. Contributing to the book's value, aesthetic appeal, and cost is a series of superb illustrations: photographs of Datini palaces, offices, and centers of operation, of business records, and of correspondence (including letters in Arabic and Hebrew), and the marks of over one hundred business firms with which Datini maintained relations.

Melis' research has not changed radically the pattern of Datini's mercantile

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activity developed by earlier scholars, nor the general picture of Italian business organization and enterprise at the end of the fourteenth century. He has, however, greatly increased our detailed knowledge of the subject, and as a result of his patient investigation and thorough documentation future scholars will be able to describe, with greater security and authority, many important dimensions of Mediterranean business activity. On certain problems, his researches are particularly informative: the analysis of profit-and-loss patterns; the study of company personnel; the detailed description via correspondence and accounting records of the transfer of a consignment of Provençal wool from Aigues Mortes to Pisa; and the exhaustive study of Datini cloth manufacture at Prato.

Melis is at his best when close to his sources, and, particularly, to the account books, the figures and statistics, the details of buying, selling, and recording transactions. He is less effective and less convincing when he proceeds from the particular to the general. Indeed, a serious defect of the work is its excessively narrow focus, its failure to incorporate the results of these detailed investigations into the larger framework of *trecento* economic history. Only rarely (pp. 302, 317, 328–31) does Melis engage in comparative analysis or place the subject of his study in a broad context. Fundamentally descriptive, this work contributes little to the formulation or the solution of the significant problems of medieval economic history.

The limitations of the author's analysis are most clearly evident in his study of Datini the man. Melis' admiration for his subject is unbounded, and his description often comes perilously close to hagiography. This adulation contrasts sharply with the negative judgments on Datini rendered by Sapori, Origo, and others, and Melis' efforts to demonstrate that his hero was a paragon of virtue are not entirely convincing. Nor is the evidence that he adduces: encomiums of the deceased merchant written by associates and employees perhaps should not be considered as impartial evidence of his sterling qualities. The analysis of the Prato merchant's character by Marchesa Origo remains a more balanced and perceptive treatment than that presented by Melis.

The evidence of Datini's business acumen presented by the author is less suspect, but doubts also arise here. Can one really accept his argument that Datini invented the system of multiple companies (*sistema d'aziende*) that was adopted with great success by the Medici, when the records of other business firms of this period have not survived? Would Datini's entrepreneurial genius seem so remarkable if we knew more of his competitors? The Datini records themselves may provide hints for establishing the identity of the largest and most successful business firms operating in these years. But like other problems outside his narrow focus, Melis does not consider the question.

With the publication of the second volume, this work will be provided with an analytical index which is sorely needed to make it usable. Detracting from the book's value and readability are the discursive character of the writing and the inchoate paragraph structure. The quality of this lengthy and ponderous work would have been greatly improved if it had been subjected, prior to publication, to

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vigorous pruning and condensation, and to a thorough overhaul of its organization.

University of California, Berkeley GENE A. BRUCKER

THE SOCIAL WORLD OF THE FLORENTINE HUMANISTS, 1390–1460. By *Lauro Martines*. (Princeton, N. J.: Princeton University Press. 1963. Pp. x, 419. $8.50.)

THE problem of defining the economic and social status of the Italian humanists has attracted the attention, if only in passing, of a number of scholars during the past hundred years. The result, however, has seldom amounted to more than loose and largely unsupported generalizations. The prevailing, though not the only, opinion has been that the humanists were on the whole struggling scholars, wandering from place to place, precariously employed and ever in search of patronage. The general tendency has been to concentrate attention on those humanists who made their living as scholars, writers, secretaries, and teachers, while ignoring those who lived on inherited wealth or combined their humanistic activity with business, politics, or a learned profession and were therefore independent of patronage. In this important book Lauro Martines offers an authoritative answer to the problem in so far as it applies to one humanist group, that which flourished in Florence during the years from 1390 to 1460, the period characterized by the full growth of that peculiarly Florentine phenomenon commonly referred to as civic humanism.

The statistical material upon which Martines bases his conclusions is drawn mostly from an exhaustive analysis of unpublished sources in the Florentine State Archives. For the wealth of the humanists he has relied heavily upon the registers of the *prestanze* and *catasto,* which list the amounts levied upon citizens for forced loans and income taxes. To establish their place in the political life of the republic he has listed all the offices held, not only by the humanists themselves, but also by members of their families, and has also examined the *Consulte e Pratiche* records for evidence of their influence on the shaping of legislation and executive decisions. Finally, as a further evidence of their social status, he has traced the marriage alliances contracted by the humanists and by members of their families. The conclusion he reaches is that, "with some exceptions, the men attracted to humanism were drawn from the ruling sectors of society—the great merchants and bankers, the administrative class, the professional class. Even the exceptions involved men most of whom swiftly found their way into these circles." Among the latter, the most important—Salutati, Bruni, Poggio—rose to wealth and position comparable to that of the majority of patrician families. The evidence is conclusive. With few exceptions, the members of the humanist circle in Florence during these years did not have to seek patronage nor to edge their way into the charmed circle of the ruling oligarchy. They belonged there by right of birth, by the possession of wealth and political office, or by achievement in the learned pro-

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fessions. That this was a unique moment in the history of Italian humanism the author is well aware. It would be hazardous to apply the conclusions drawn here without qualification to the humanist movement as a whole. Within its limits of time and place, this book is, nevertheless, an invaluable addition to our knowledge of the humanists and of the society of which they formed an integral part. Not the least interesting aspect of this study, indeed, is the analysis in the introductory chapter of the factors that determined social status in Florence. The detailed lists of public offices held by humanists and their families, too, furnish the most concrete evidence I have seen of the degree to which members of the patrician oligarchy devoted their time to the business of government.

University of Western Ontario WALLACE K. FERGUSON

Modern Europe

DIE RÖMISCHE KURIE UND DER WESTFÄLISCHE FRIEDE: IDEE UND WIRKLICHKEIT DES PAPSTTUMS IM 16. UND 17. JAHRHUNDERT. Volume I, PAPST, KAISER UND REICH, 1521–1644. Part 1, DARSTELLUNG. By *Konrad Repgen.* [Bibliothek des Deutschen Historischen Instituts in Rom, Number 24.] (Tübingen: Max Niemeyer Verlag. 1962. Pp. xlv, 555. DM 68.)

THIS is the first in a projected three-volume study of the relationship of the papacy to the Peace of Westphalia of 1648. Its subject is the relations between Empire and papacy from 1521 to the beginning of the Westphalian negotiations in 1644. A second volume will assess the position of the papacy in Europe as a whole for roughly the same period; the final volume will discuss the policy of the Holy See in Münster from 1644 to 1648, the papal breve *Zelo domus dei* of 1648, and the later history and significance of this breve.

The present volume is more than "background" for Volume III of the series. It has its own theme: "what position the papacy took toward the religio-political and religio-juridical consequences of the [Reformation] in Germany from 1521 to . . . 1644." Basic to this problem is Repgen's proof that in spite of numerous oral admonitions it was not until the 1640's that the papacy authorized a formal written protest against imperial religious policy—that of the nuncio Mattei against the Amnesty of 1641. The question then arises: since the principle of confessional parity had been recognized legally in the imperial estates since the sixteenth century, why did the papacy wait until 1641 to protest formally against an imperial religious policy which for more than a century already had been conceived as prejudicial to the Catholic Church?

Repgen's answer is not a simple one, and it involves a survey of papal policy at certain key points in the development of the German religio-political constitution, beginning with Luther's hearing at Worms. Among the great questions of

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this time was whether, under greatly altered conditions, the medieval relationship of Church and Empire could be maintained. Until 1532 the form of the medieval union of canon and imperial law was kept, though not enforced. Thereafter, even the formal union was broken. Political necessity, the changing personnel of pontificates and nunciatures, and ignorance of policy precedents (a fascinating aspect, to which Repgen devotes much attention), combined with a desire to maintain strict canonical correctness, led the Church to walk a tightrope between approval of concessions to Protestantism, on the one hand, and formal, binding disapproval on the other hand, which might have prejudiced Catholic interests and perhaps have created schism among German Catholics, placed in a tug of war between political circumstance and Church doctrine. When formal protest was made, in 1641, it came on an unimportant issue, too late to influence practical affairs, and served merely to bind the papacy to an increasingly unrealistic policy.

In addition to its special contribution to the above issues, this book also illuminates the general problem of secularization and tolerance as represented in the conflicting claims of politics and religion within a specific state structure: the vitally changing meaning of the Empire is sharply pointed up in this story of the progressive redefinitions forced upon Emperor, imperial estates, and papacy by the events of the Reformation.

This study is an important addition to the literature on papal German policy in the Reformation period. Scrupulously documented, it contains large amounts of documentary text in the footnotes, much of which has been unavailable since the suspension of the *Nuntiaturberichte* in 1897. My only major criticism of this otherwise solid book lies in its treatment of papal relations with Germany largely apart from the wider international position of both Empire and papacy. True, Volume II is intended to explore that area, but in reading this volume, one is scarcely aware that papal imperial policy was influenced by non-German events also; their absence from this volume throws the reader too much on his own devices for this added dimension.

Amherst College JOHN G. GAGLIARDO

THE UKRAINIAN–POLISH PROBLEM IN THE DISSOLUTION OF THE RUSSIAN EMPIRE, 1914–1917. By *Oleg S. Pidhaini.* (New York: New Review Books. 1962. Pp. 125.)

POLISH–SOVIET RELATIONS, 1932–1939. By *Bohdan B. Budurowycz.* [East Central European Studies of Columbia University.] (New York: Columbia University Press. 1963. Pp. xi, 229. $6.00.)

THESE two volumes have one thing in common: they both treat complex problems of Eastern Europe. The similarity, however, ends there. Pidhaini's slender volume "is meant to be an introduction" to a "wider work on the rebirth of the Ukrainian commonwealth in the Revolution and the diplomacy of European powers, 1917–1920." The author's intent is admirable (for there is a need for such

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a study), but the adjective cannot be used to describe the present volume. The three brief chapters that comprise this "introduction" are somewhat superficial, include too many long quotations, and, while documented, reveal the author's unawareness or neglect of a number of documentary collections and of such studies as those by John S. Reshetar, Jurij Borys, and Richard Pipes, to name a few. It is hoped that in preparing his "wider work" the author will take these weaknesses into consideration in order to provide his readers with a depth that is lacking in the present work.

The study of *Polish-Soviet Relations, 1932–1939,* by Budurowycz, in contrast, has all the earmarks of sound scholarship and sober analysis. The main thought running throughout the entire work is that in the period under consideration the prime goal of Polish foreign policy was centered on resolving a major dilemma: how to uphold Poland's freedom and territorial integrity in the face of the rising might of Nazi Germany and Soviet Russia without siding definitely with either of its two powerful neighbors. Relying chiefly on primary sources, the author shows clearly and quite persuasively that the Polish policy of balance between Germany and the USSR was pregnant with grave dangers. The main reason for these dangers was that Polish attitudes of nonalignment were misinterpreted both in Berlin and Moscow. In their attempts to maneuver between Germany and Soviet Russia, moreover, Polish leaders made a number of mistakes and errors of judgment, underestimated the Nazi danger, and in the crucial days before "the Fourth Partition" minimized the chances of Soviet-Polish cooperation.

This study of Polish-Soviet relations, though not exhaustive, is sound in its basic conclusions. These conclusions would have been considerably strengthened, however, had the author given more attention to the Polish involvement in the dismemberment of Czechoslovakia, and especially the impact that involvement produced on Polish-Soviet relations. Missing also are the intricate Nazi-Soviet "feelers" between October 1938 and March 1939. While these omissions are unfortunate, it must be stated that this is a study of value and merit. Students of Eastern Europe and European diplomatic history will find in it much new information and will also profit from the twenty-page bibliography.

Portland State College BASIL DMYTRYSHYN

DER HITLER–STALIN–PAKT, 1939–1941: EIN BEITRAG ZUR METHODE SOWJETISCHER AUSSENPOLITIK. By *Philipp W. Fabry.* (Darmstadt: Fundus Verlag. 1962. Pp. 535.)

IN this detailed study based mainly on published materials, Fabry attempts to present a new picture of German-Soviet relations. According to Fabry, Adolf Hitler was confronted by the danger of war from the West after Munich and could remove the threat from the rear by Poland either through friendship with Poland or a pact with Russia. Since Poland refused friendship, and Japan declined an alliance against Britain, Germany turned to Russia. The blame for the out-

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break of war in 1939 rests with Russia for its decision to encourage Hitler; similarly, the Soviet Union was responsible for the German attack in 1941 because of its pursuit of Russian ambitions in the Balkans.

This picture is drawn by combining dubious assertions with strange contortions of the evidence. Hitler is supposed to have given up all his eastern ambitions after 1933. The Nazi-Soviet pact was allegedly designed by Hitler to assure peace with Poland. German ambitions in 1939 not only did not extend beyond Danzig, but even that territory was supposed to be merely the subject of a European conference. Fabry frequently contradicts himself. For example, he states that the Soviet Union moved forward in October 1939 because of the possibility of immediate peace, after maintaining just the opposite. When it suits his scheme, he pictures Hitler as worried about American armaments; when it does not, Hitler is unconcerned. The discussion of Hitler's view of Stalin's Russia is preposterous: Fabry is astonished that so few people have noted that by the secret protocol to the nonaggression pact Hitler conceded nothing beyond the borders of tsarist Russia before 1917. I am astonished at Fabry's ignorance of the pre-1917 ownership of Galicia.

Where the sources do not fit into Fabry's picture, they are ignored or subjected to operations. Where the secret protocol to the nonaggression pact refers to "these areas" in Southeast Europe, Fabry makes the words singular to make them apply only to Bessarabia. In quoting from the Halder diary for the famous conference of July 31, 1940, he omits the reference to Rumania so that related decisions of Hitler can be postponed a month. In citing a letter of General Warlimont on the July 1940 conferences in German headquarters, Fabry omits a pagelong recital of reasons given by Warlimont to support his belief in a decision by Hitler in July 1940 to attack Russia, so that Warlimont can be converted into a leading witness for the opposite viewpoint. One further example illustrates Fabry's peculiar handling of documentary evidence: Hitler's directive Number 18 of November 12, 1940. Those parts of the document that suit Fabry are characterized as "Hitler's views," and with the phrase "here Hitler expressly stated." A passage of the same document that does not fit Fabry's preconceived scheme can be dismissed because it was merely "a formulation of Warlimont . . . not of Hitler."

If the handling of German sources is questionable, that of Soviet sources has the merit of simplicity. All Soviet evidence that has become available in the last decade is ignored or declared nonexistent. The "contribution to the methods of Soviet foreign policy" promised in the subtitle is, therefore, rather slim.

There can be no doubt that a thorough reworking of the materials now available on the period of the German-Soviet pact could have been a valuable contribution to diplomatic history. Rigid preconceptions and methodological inadequacies have unfortunately prevented this work from providing it.

University of Michigan GERHARD L. WEINBERG

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ABSTRACT OF BRITISH HISTORICAL STATISTICS. By *B. R. Mitchell,* with the collaboration of *Phyllis Deane.* (New York: Cambridge University Press. 1962. Pp. xiv, 513. $10.00.)

WHATEVER their views on whether their profession is a science or an art, historians have in the past generation imitated the physical sciences in their growing preoccupation with quantitative data. Conclusions that can be expressed numerically seem, if not more true, at least more satisfying than qualitative statements. The day is past when the judgments of a More or a Cobbett could be regarded as adequate indications of the nature and extent of enclosures. The search for increasingly exact statistical information is transforming political and social as well as economic history. While students of the late Sir Lewis Namier count cousins, sinecures, and votes, Peter Laslett and his associates at Cambridge are re-examining Stuart society on the basis of scattered parochial censuses. Our views on the impact of the industrial and agricultural revolutions on eighteenth-century life may require serious revision when J. T. Krause completes his investigation of baptismal and burial records.

The monumental production of Miss Deane and Dr. Mitchell ought to encourage historians to make further use of statistical evidence to illuminate other aspects of the British past. A by-product of the work of Deane and W. A. Cole on long-term British economic growth, its scope encompasses population and vital statistics, the labor force, agriculture, mining, manufacture, transport, building, overseas trade, wages and the standard of living, national income and expenditure, public finance, banking, insurance, and prices. Although there is one entry for as early as 1199, giving the output of white tin in Cornwall, few of the series begin earlier than the eighteenth century, and most of the statistics come, as might be expected, from the nineteenth and twentieth centuries, ending, with a few exceptions, in 1938.

Beautifully produced and lucidly organized, the work will both facilitate the investigations of the specialist and make it less excusable for the noneconomist to ignore the available statistics relating to his field. Because much of the contents has not been readily available in print, and a great deal of that made misleading by errors of transcription and arithmetic, Deane and Mitchell have performed a noteworthy service in bringing together and reworking their raw material. The historian will be particularly grateful for the admirable description and critical evaluation of the statistics to follow, which precede each of the sixteen sections, and of the illuminating notes that accompany the tables. The authors wisely quote L. S. Presnell in warning against "the temptation to use faulty statistics because no others are available" and are careful to indicate the varying degrees of reliability of the different series and the ways in which they may and may not legitimately be used. At the end of each chapter a bibliography lists works that discuss and criticize the statistics included, as well as sources for further statistics that for one reason

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or another, usually severe doubts as to their accuracy or usefulness, the authors have chosen not to reproduce.

The work will be indispensable to anyone whose research or teaching involves modern British economic history.

Vassar College DONALD J. OLSEN

ESSAYS ON THE SCOTTISH REFORMATION, 1513–1625. Edited by *David McRoberts*. (Glasgow: Burns. 1962. Pp. xxix, 496. 70*s*.)

ONE of the notable aspects of recent historical study has been the very considerable literature by Roman Catholic scholars devoted to a re-examination of the sixteenth-century religious struggles. While not all of these writers achieve the distinction of a Joseph Lortz, nonetheless the trend is significant. This volume is a reprint of a series of articles that appeared in the influential Roman Catholic quarterly, *The Innes Review,* of Glasgow, during the four hundredth anniversary year of the Scottish Protestant Reformation, 1959–1960. The authors are mostly Roman Catholics, plus one or two Anglo-Catholics. The editor states that the series was planned as an attempt to re-examine objectively the period 1513–1625.

The range of subjects dealt with in the fifteen essays is broad. The political situation over the century, the hierarchy, the monastic orders, the priesthood, education, care of the poor, doctrinal controversies, the fate of Church property in the clash of interests, persecution for religion, and Scottish relations with Rome are the major topics treated. The uneven quality of the articles is inevitable in any such work.

The picture that these essays present of Scotland during the years 1513–1625 is not pleasant. It is that of a small, strife-torn nation caught in the rivalry between England and France. During most of the century the country was close to anarchy because of the weakness of the crown and the crude, shortsighted, and vicious nobility. Toward the close of the era the crown, with the support of the rising middle class, first began to achieve a stable civil existence. The opinion is expressed that, with the power struggle as it was, Scotland could not under any circumstances have pursued an independent or neutral policy in the English-Protestant versus French-Catholic rivalry. Throughout the volume the judgment runs that not France, nor the French monarchs and politicians, not England, nor the English monarchs and politicians, not the Stuart monarchs, nor the Scottish nobility, had any serious interest in religion save for its usefulness in their purposes. This view is regarded as an important contribution of the volume.

Several of the essays attempt to re-examine the traditional condemnation of the Roman Church in Scotland during this period. Here the attitude of scholars such as Lortz is often (though not always) found. There is little apology for the deplorable situation, and there is some sober statistical demonstration of the degradation of the Church. Over 90 per cent of the parishes were appropriated benefices. The hierarchy were as corrupt as any in Europe. The lower clergy were poor and

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demoralized. The monastic orders were overextended and without much sense of vocation. Yet each of these ranks showed at least some efforts at self-reform. Uniformly, the crown, the ecclesiastical powers, and even the papacy betrayed these reform efforts in the interests of power, money, or other pelf.

Several long-standing positions are rejected in these essays. The class interpretation of the struggle between Catholic and Protestant is called "too simple." Of persecution for religion it is said that there was less on both sides than has usually been claimed. On the destruction of Church property it is said that the motives for this vandalism were so varied over the century that common judgments must be revised. The essays dealing with theological issues are among the weaker papers. There is little understanding here of what the Protestants thought, and the essays attempt no re-examination of these matters.

As a whole the volume is a good contribution to Scottish history. It does not greatly revise the judgments of recent non-Catholic scholars, but it does round out the picture in many ways. Most of all, it represents a very welcome development in historical study.

San Francisco Theological Seminary L. J. Trinterud

ISAAC NEWTON, HISTORIAN. By *Frank E. Manuel.* (Cambridge, Mass.: Belknap Press of Harvard University Press. 1963. Pp. viii, 328. $7.50.)

Sir Isaac Newton is finally beginning to receive the attention he deserves. There have been, in the past, a few scattered biographies, but, by and large, the enormous mass of manuscript material and published works that Newton left behind have not been exploited. Scholars are now beginning to sort, classify, edit, and publish these works, and we may look forward soon to a real appreciation of Newton's life and work. Manuel has courageously tackled the problem of making sense out of Newton's historical and chronological scribblings. All Newton scholars will be in his debt for he has charted a valuable course through this particular manuscript jungle.

The core of Manuel's book is his discussion of Newton's *Chronology*. By utilizing the precession of the equinoxes and making a few highly speculative guesses about astronomy in Greek antiquity, Newton was able to chop five centuries off the standard ancient histories of his time. This, in turn, permitted him to elevate Israel to a position of chronological primacy and, thereby, vastly increase the value of the Old Testament as a source of ancient history. To Newton, the Hebrews were the fount of all wisdom, and he could not bear the thought that other peoples might have invented something before them. Manuel discusses Newton's chronology and its reception in detail. He also treats Newton's historical method and the ways in which he wrestled with the contradictions and problems raised by his own ingenuity.

Manuel does not restrict himself to a mere summary of Newton's historical efforts. He has his own thesis as well. He tries to show that both Newton's scien-

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tific and historical works spring from the same basic ideas, in spite of Newton's repeated remarks to the contrary. This forces him to find "science" where none exists, and the thesis is never really satisfactorily supported. The *Chronology,* to be sure, is based upon the precession of the equinoxes, but this fact had been known for centuries and had nothing whatever to do with Newtonian science. Newton was undoubtedly in a good position to use it in his historical investigations, but this gives little support to the main thesis. Similarly, terming the identification of Sesostris with Sesac an *"experimentum crucis"* merely introduces a scientific notion where it does not belong. It is a crucial point for Newton, but such points exist even in nonscientific history. Finally, the identification of Newton's account of the growth of monarchies with the *Principia* and the suggestion that both are examples of the application of the principle of universal attraction are quite farfetched. It seems obvious that Newton was simply trying to work his material into accord with the Book of Daniel.

Curiously, Manuel neglects to mention the one important point where history, as Newton saw it, did contribute to the new science. As so many of his contemporaries had done, Newton identified Moschus the Phoenician with Moses. Moschus, according to an ancient tradition revived in the seventeenth century, was the originator of the atomic theory, and, by turning him into Moses, the atomic theory upon which the mechanical philosophy relied so heavily could be preserved from the criticism that atomism and atheism were synonymous.

Manuel's book is, then, a valuable one in so far as it shows the way to further investigators, but its claims for Newton are somewhat extravagant and should be treated with some skepticism.

Cornell University L. Pearce Williams

EARLY ENGLISH LIBERALISM: ITS EMERGENCE THROUGH PARLIAMENTARY ACTION, 1660–1702. By *George L. Cherry*. (New York: Bookman Associates. 1962. Pp. 325. $6.00.)

In a brief preface to the four parts and seventeen chapters of his book, the author declares that during the period under consideration "fundamental decisions were being forged." Out of these the "foundations of modern liberal society were created. During this era the features of political, economic, religious, social and individual liberalism were emerging. Many of the ideas considered novel in 1789 were in reality commonplace in 1680." These rather sweeping claims for the achievements of the Parliaments of the later Stuarts are reiterated in one form or another at the conclusion of each part—"Background," "Political," "Religious," and "Economic Liberalism," respectively—but there is no other general summary.

Standard authorities have been used: printed debates are quoted; an unnecessarily detailed listing of the *Calendars* in the bibliography suggests more importance than they actually have; pamphlets are virtually ignored; statutes are derived from Andrew Browning's *Documents*. For the purposes announced this perhaps

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does not matter, but at times a tract like Peter Paxton's *Civil Polity* might have bolstered arguments about the existence of economic liberalism more effectively than the sources referred to.

Mr. Cherry presents an interpretation of the policies of the period and in no sense claims to offer an analysis of new or esoteric material. The book then must be judged on its thesis and the case for it. No sane historian could deny some of the facts used by Cherry. Parliament certainly increased its power in 1688; the Toleration Act of 1689 permitted some Englishmen to worship more freely; mercantilist theories and practices were attacked and even modified as the seventeenth century ended. The destruction of "obstacles standing in the way of freedom" preceded the "erection of bulwarks" guaranteeing individual rights. So much every Whig, or Tory, or Namierite historian would agree to, but whether all would concede that the Revolution "produced the foundations of the modern liberal state" is more than doubtful. Parliamentary supremacy was achieved, but the nature of Parliament, its relationship both to king and to people was far from clear for another century and a half at least. Nor can the opening, in a limited way, of trade by Parliament be altogether credited to a new and firm economic liberalism.

The book contributes nothing to our knowledge of the period that is not readily available in the textbooks of the period. It adds confusion to already confused concepts of liberalism. It leaves much unanswered, and we are often left somewhat vague as to the author's purpose. A vigorous defense against some modern deprecatory accounts of the achievement of Englishmen in the late seventeenth century might have illuminated the nature of the Revolution. An examination of the literature might have clarified the extent to which Englishmen were aware of those "commonplaces" to be voiced in 1789. Cherry avoids the famous debate (in 1689) in which Mr. Sawyer criticized the Convention Parliament and from which we may derive a rather less than "liberal" view of Whig politics. He avoids a definition of "the people." The mind of the seventeenth century is enormously fascinating, and much of its theory and philosophy should still be studied and interpreted, but perhaps we might leave aside the word "liberalism" till we agree on a precise definition.

Bryn Mawr College CAROLINE ROBBINS

WILKES, WYVILL AND REFORM: THE PARLIAMENTARY REFORM MOVEMENT IN BRITISH POLITICS, 1760–1785. By *Ian R. Christie.* (New York: St Martin's Press. 1962. Pp. xi, 247. $5.75.)

FIFTY years ago, G. L. Veitch published his *Genesis of Parliamentary Reform,* a noble beacon of pre-Namierite scholarship. In the then dominant (and still pervasive) Protestant, liberal mode, he studied not the establishment itself, but its critics and would-be improvers. His study of the first stirrings of the parliamentary reform movement in eighteenth-century England was scholarly, warm-

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hearted, committed, present-minded, then and since a delightful guide for the student who wants a side to cheer for. Today, in a colder, post-Namier light, scholars are beginning to look closer at Veitch's heroic canvas. G. F. E. Rudé in his *Wilkes and Liberty . . . 1763 to 1774* has recently examined most suggestively the social bases of urban radicalism, the composition of the mass that made up Wilkes's street mobs and electorate (analogous to Rudé's similar studies of the street mob in the French Revolution). Now, Ian Christie gives us a fresh, learned, more or less Namierite rethinking of the succeeding reform agitation of 1779–1785, with particular emphasis on Christopher Wyvill, the Yorkshire Association, and the respectable side. The coverage of his book is actually somewhat more restricted than the title would imply.

The Namierite quality of Christie's book is at first rather elusive. It is not heavily analytical, but rather a straightforward narrative. He has, of course, already analyzed the Parliament of 1780–1784 in *The End of North's Ministry,* but the county and other associations could perhaps stand a somewhat more elaborate delineation. Nor is his work Namierite in its laborious exhaustion of all documentation and all ways of looking at a phenomenon. He has used new papers of Wyvill and others, but there still remains room for further work on the reformers of 1779–1785, particularly in the metropolis, utilizing as they become available the papers of Shelburne, Charles James Fox, and others. Nevertheless, Namierite his work most profoundly is, alike in its intellectual conception, its freedom from anachronism, and its emotional discipline. Christie looks steely eyed and detached at the agitation of 1779–1785 and finds it, for all its intimations of 1832, a pretty insubstantial affair, wordy and unfocused, too ideologically extreme in the capital, too tactically cautious in the provinces to strike fear in the hearts of any government. Characteristically Namierite is his refusal to discern what Professor Butterfield would regard as "the higher purposes of the Rockingham Whigs"; nor has Christie had the grace to find much consistency or honesty in Charles James Fox. His Younger Pitt is a little more straightforward than that of Donald Grove Barnes, but no more effectual.

Fifty years ago, Veitch found us a hero in that "modern" ideologue and do-gooder, Major Cartwright; today, Christie finds his, if at all, in the rather old-fashioned figure of the parson-squire, Christopher Wyvill. Yet Wyvill not unexpectedly makes a most dubious hero, for, in building his movement upon gentlemen led by neighbors, he charted its immediate success and ultimate failure.

University of Michigan JACOB M. PRICE

GREAT BRITAIN AND SEA POWER, 1815–1853. By *C. J. Bartlett.* (New York: Oxford University Press. 1963. Pp. xviii, 364. $6.40.)

THE author, who is on the faculty of University College of the West Indies in Kingston, Jamaica, rightly argues that 1815–1853 is "one of the most neglected periods of British naval history." What he has attempted is "a synthesis, not of

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all naval history for the period, but of the main forces determining British naval policy." He succeeds in this purpose admirably, and with faultless scholarship. The array of sources used is impressive, notably extensive, unpublished primary source materials in the Public Record Office, the British Museum, and the National Maritime Museum at Greenwich.

Bartlett deals with three main themes. One is the debate in the cabinet, the Admiralty, and Parliament over the size and character of the British Navy. The leitmotiv here is the clash between "the alternating and erratic pressures of foreign scares and domestic economy." The former was a product of the Russian and French naval challenges, chronic French invasion scares, and the revolution in naval matériel toward mid-century—the introduction of steam navigation and shell guns, which threatened to nullify British superiority by rendering existing warships obsolete. In the contest between fright and parsimony the former usually won. The second principal theme is the use of the navy in power politics. Not that a British squadron often had to fire a shot in anger. "We are," declared Palmerston in 1836, "fighting a Battle of moral influence in Turkey and Egypt, and the strength of Rowley's squadron is a material element in our moral influence." For good illustrations of how the fleet assisted in the implementation of foreign policy, see how Palmerston used it in the 1830's to serve his goals in the Low Countries, Spain, Portugal, and the Near East. (Bartlett, incidentally, assumes his readers have a fairly detailed knowledge of the diplomatic history of the whole period.) The third theme, which receives a more summary treatment, is the navy's work in the struggle against the slave trade and in the promotion and protection of British interests overseas. Palmerston justified gunboat diplomacy in these classic terms (1850): "These half-civilized Governments . . . all require a dressing every eight or ten years to keep them in order. Their minds are too shallow to receive an impression that will last longer than some such period and warning is of little use. They care little for words and they must not only see the stick but actually feel it on their shoulders before they yield to that only argument which to them brings conviction, the *Argumentum Baculinum*." The book concludes with a fine "portrait" of the navy on the eve of the Crimean War.

The bibliography is full, the index excellent, and the simple maps quite adequate. If the volume has any weaknesses, they lie in its style and organization. Occasionally the detail piles up into a formidable mass, and, admitting the difficulty in achieving it, there is not an entirely happy marriage between the chronological and topical approaches. It is to be hoped that Bartlett will give us a comparably useful study of the Royal Navy in the post-1853 generation.

University of Hawaii ARTHUR MARDER

THE FACTORY MOVEMENT, 1830–1855. By *J. T. Ward*. (London: Macmillan and Company; New York: St Martin's Press. 1962. Pp. xi, 515. $12.00.)

IF detail is synonymous with depth then this is indeed a profound work, con-

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sisting as it does of almost five hundred pages of copious narrative and notes describing the origins, organization, structure, vicissitudes, and achievements of the factory movement in Britain. Ward's intention is to present for the first time a complete history of the movement, building upon the solid foundations of work done by Driver, Thomas, and others. There is also some attempt to assess the movement's influence in the wider context of political organization and philosophy.

Ward's major conclusions consist of a denial that self-interested motivation produced parliamentary divisions over factory reform; he argues that political rather than economic considerations were paramount. He supports Dicey's view that humanitarianism was the progenitor of the factory movement and that the forces giving this movement its strength included the strong Tory penchant for paternalism and Anglican evangelicalism, which elements blended and helped to produce a new radical tradition in conservatism. The movement, it is stressed, was constitutional, orderly, and restrained, despite the vocal bluster of its advocates. Its legacies include the origins of Christian social doctrines and a greater sophistication in the organization of protest movements.

Few of the author's conclusions depart from accepted views on the nature and significance of the factory movement as propounded by Driver, Dicey, and Brebner, but he adds to the already abundant literature on working conditions during this period and also deals most thoroughly with the influence of Anglican evangelicalism. Ward succeeds in delineating the relationships between the movement and other socioeconomic and political protest groups and indeed considers that the essential value of his contribution lies in presenting for the first time the history of the movement in its entirety. It is, in effect, a painstaking chronological and detailed description of local and national political maneuvers, campaigns, and elections, with abundant documentation of the polemics of factory reformers and their opponents. One might suspect, however, that the spadework involved in turning over mountains of local sources left the author with much material he was reluctant to omit, which, while suggesting that the author might accept implicitly a kind of labor theory of value, also exposes the reader to the risk of experiencing diminishing returns.

University of British Columbia ROY A. CHURCH

FABIAN SOCIALISM AND ENGLISH POLITICS, 1884–1918. By *A. M. McBriar*. (New York: Cambridge University Press. 1962. Pp. x, 387. $9.50.)

IN his preface the author tells us that his book "is concerned mainly with Fabian doctrine and only incidentally with the history of the Fabian Society or with the Fabians as individuals." There is an excessive modesty in this claim. While the book is in no sense a conventional history of the society, and much less a detailed study of its leadership, both in fact receive here as dispassionate and critical a review as is to be found in any other work. It would be difficult to cite a better introduction to the Fabian Society than this one, or another so free of

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partisanship and so lacking in the traditional awe that have been characteristic of so many other works on this subject.

McBriar, lecturer at Melbourne University, comes to the Fabian Society from outside. He is not overwhelmed by what it has accomplished, but he is not at all disposed to underrate its achievements. Paying particular attention to the *Tracts* and other Fabian writings, he describes in detail the Fabian contributions in developing a distinctive economic theory, sociology, and political philosophy. This part of the work reminds one of the late Professor Schumpeter's *Capitalism, Socialism, and Democracy;* what Schumpeter did for Marxism, McBriar does for Fabianism. This is a most useful summary. The author is as much concerned to describe the intellectual origins of Fabianism as he is to describe the contributions made by individual leaders of that society.

Since his interest is not only with the ideas but also with Fabianism as a political movement, he gives some attention also to the specific economic, imperial, and diplomatic problems of the late Victorian and Edwardian periods. This is a mere summary statement and does not go much beyond what is available in other sources. Where the author makes a more important contribution is in his last four chapters, where he discusses, successively, the Fabian influence on the London County Council, on the Liberal party, on the Independent Labour party, and on the Labour party. This is a remarkable summing up of what the society sought to do, what it in fact accomplished, and why it did not do more. The quality of McBriar's criticism may be suggested by his final remarks on the Fabian influence in the London County Council. He writes: "The Fabians deserve a place of honour amongst those men who converted London from one of the worst-governed cities of the British Commonwealth, which it was in the 1880's, to one of the best governed, which it is today. Their success, however, was greater as administrators within the scope of the Progressive Party's programme, than as propagandists for the extension of municipal activity in new, socialist directions."

The story of the society's growing disillusion with the Liberal party is well told, as is that of its relations with the Independent Labour party. This last theme has not been much studied, and McBriar's chapter suggests that there are interesting possibilities for analysis here. The work is a model of what such a book should be; it is filled with specific data, intelligently and skillfully organized, and calculated to support modest but important general conclusions. The impulse to praise is as absent as any concern to condemn. This is history rather than autobiography; in this particular field, where the other prevails, this is a most welcome volume. It will be much used by scholars interested in British Socialism, its theory and practice.

Harvard University STEPHEN R. GRAUBARD

ARMED FORCES IN PEACETIME: BRITAIN, 1918–1940, A CASE STUDY. By *Robin Higham*. (Hamden, Conn.: Archon Books. 1962. Pp. viii, 332. $10.75.)

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In the years between the wars the British armed services were the stepchildren of successive governments and the target of a continuous campaign of economy, and they were plagued by depression, indifference, hesitation, and the running down of industrial capacity. Higham's account of these matters is useful but disappointing. Its usefulness is that of a pioneer work, based on the very extensive literature of military and naval memoirs (their number will astonish the layman), professional books and journals, and the relevant volumes of the current "Civil Series" of the official British history of the Second World War. It is disappointing partly because it fails in its professed objective of providing a case study of the neglect of the armed services in peacetime, attempting no comparisons with the experience of other countries, such as France, and partly because of its graceless writing and its occasional errors and obscurities.

Some of Higham's expressions are merely unfortunate or repetitious, such as his habit of placing a string of epithets in front of a person's name: "publicity-conscious, prime-ministerially aspiring, smooth politician Walter Long" is one example; Hoare is called suave twice on one page. Some are dubious: was "unemployment encouraged by the dole"? Some are erroneous: one gasps to read that Lord Hailsham was a "West India sugar merchant with . . . no previous governmental experience" in 1931, when he had already been attorney general and Lord Chancellor; Ribbontrop [*sic*] was not Foreign Minister in 1935. There are many solecisms: prestigious, dichotomous, detest (as noun), disinterested (for uninterested), "so badly decimated," careerwise, decisionless (for indecisive, and hardly true of Ramsay MacDonald), subverted (for subservient), vindictive (for self-vindicating). Other words are used in an individual and unexplained sense: materialist (noun), teleological war, pacifist (applied to Neville Chamberlain and others, apparently for seeking peace).

The weakest chapters are at the beginning and the end of the book. The first, on demobilization and on intervention in Russia, is sketchy, and the second, on the armed forces and civil affairs, is so compressed as to be positively misleading in its references to strikes in 1919–1921, the war in Ireland, and the Chanak incident. The last deals with politicians and defense and has no difficulty in showing that the service ministries were usually held by second-rank men without special experience or the time in office to acquire it. It is not necessarily experience, however, that matters, as Hore-Belisha's term at the War Office shows, and the influence of the civil servants at the head of the service departments deserves but perhaps defies more study.

In between, there are four chapters that are lively and full of interest, describing in detail how the army and navy fared in the doldrums, how the RAF preserved itself as a separate force, and how rearmament proceeded at uneven pace. Here are the book's heroes (though presented without hero worship): Lord Trenchard, Chatfield, Martel, Admiral Richmond. The self-inflicted weaknesses of the services are fairly exposed: "antediluvian generals" at the War Office, the Admiralty's neglect of the dangers from submarines and aircraft, and the RAF's reliance for

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defense on the "counter-strike-force" of the bomber, only to find in 1937 that it had no bomber force which could strike with any effect against the obvious enemy.

University College of North Wales C. L. Mowat

RICHELIEU AND THE COUNCILLORS OF LOUIS XIII: A STUDY OF THE SECRETARIES OF STATE AND SUPERINTENDENTS OF FINANCE IN THE MINISTRY OF RICHELIEU, 1635–1642. By *Orest A. Ranum*. (New York: Oxford University Press. 1963. Pp. vi, 211. $5.60.)

Three important books on French administrative history by non-French scholars have recently appeared in rapid succession: Franklin J. Pegues, *The Lawyers of the Last Capetians* (1962), Miss N. M. Sutherland, *The French Secretaries of State in the Age of Catherine de Medici* (1962), and now Orest A. Ranum, *Richelieu and the Councillors of Louis XIII*. These works have much in common. All analyze the functioning of the upper reaches of the royal administration during periods when much basic research remained to be done. All approach the problem through a study of the officeholders and only incidentally the offices themselves. And all are based on extensive study of the manuscript sources. They are therefore illustrative of the high quality of scholarship that may be achieved by British and American historians who are able to devote very considerable time to research in the French archives.

Unlike the other two works, Ranum's study is devoted to a period when the royal administration was dominated by a powerful First Minister, in this instance Cardinal Richelieu. The book therefore assumes two aspects. First, it is a study of certain key offices and the men who held them: the Secretaries of State, Chavigny and Sublet de Noyers, and the Superintendents of Finance, Bullion and Bouthillier. In its analyses of the powers and development of these offices, the book is a pioneer study in many ways. The emphasis, however, is rightly placed upon the individual roles of the four men. All emerge as distinct personalities whose influence in government was determined as much by their family connections, personal merit, prestige, and, above all, their status as creatures of the cardinal as by their legally defined powers. Thus the book is, secondly, a valuable study of the methods that Richelieu used to build and maintain a personal following within the royal administration. Ranum shows that although Richelieu was continually dependent on royal favor, he frankly relied on hand-picked creatures in high office as the indispensable basis of his control over practical matters of government. So effective was the system that it enabled him to abandon the cause of administrative reform and to pursue very ambitious policies during the war years.

The book is therefore not a study of official policies and their formulation during the cardinal's last years, but is instead a study of the administrative and personal factors that strongly influenced policy decisions. Ranum repeatedly takes the rather dubious position that the King's legal possession of all sovereign au-

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thority and the nature of the surviving source materials make it impossible to identify the roles of Louis XIII, Richelieu, and the lesser ministers in determining policy. On the other hand, the book is rich in its analyses of the complex factors that influenced the making of policy and demonstrates the author's extensive knowledge of these administrative and personal realities. Particularly vivid are his handling of Chavigny's role as Richelieu's "contact man" with Louis, Gaston d'Orléans, and lesser figures, and Bullion's personal manipulation of official financial dealings. In my opinion, the section on finances makes a most important contribution to an understanding of the war years of the reign. The book is also a valuable study of the power relationships, administrative techniques, and the abilities and contributions of certain key administrators during these years of crisis. It is indispensable to all who would understand the administrative and political realities of the period and the ways in which they both limited and were utilized by Cardinal Richelieu.

Brown University WILLIAM F. CHURCH

CONTRIBUTIONS À L'HISTOIRE DÉMOGRAPHIQUE DE LA RÉVOLUTION FRANÇAISE. By *M. Bouloiseau et al.* [Commission d'Histoire économique et sociale de la Révolution, Mémoires et documents, Number 14.] (Paris: the Commission. 1962. Pp. 175.)

IN a brief introduction to the six studies included in this volume, Marcel Reinhard explains that the general purpose of this and similar works is to determine how demographic factors influenced the course of the Revolution and how the Revolution influenced the demographic history of France.

In the first of two articles dealing with the prerevolutionary period, Y. Le Moigne, in a study based on records in Strasbourg's Municipal Archives, presents convincing evidence that birth, marriage, and death rates in eighteenth-century Strasbourg are functions (within limits) of the price levels of wheat and rye. The city's population jumped from 30,000 in 1700 to 50,000 in 1789, but the means of subsistence did not increase proportionately. Consequently, wheat and rye were expensive between 1770 and 1789, and Le Moigne sees a direct relationship between high prices for grain and a stagnant Strasbourg economy in which low wages and unemployment were the rule for the two decades preceding the Revolution. (In a general way, Le Moigne's conclusions support the depression thesis of Labrousse.) Marc Bouloiseau, using Haut-Normandie as his example, concludes that population estimates based on the *feu* (hearth, family, household) must be made with caution since its size and structure varied with the region, the period, or even the social class or tax under consideration. In the collection of the taille, for example, the *feu* included only the father, mother, and minor children. But in the administration of the gabelle, the *chef de famille* was responsible for all persons living under his roof, and the *feu* might include his parents, brothers, sisters, and servants. In the first of the volume's contributions dealing with the

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revolutionary era, Jean Ibanés, exploiting records of the census decreed by the National Assembly in 1791, classifies and analyzes the population (4,148 inhabitants) of a small section of Paris (Place des Vosges and neighborhood) in terms of birthplace, age, family structure, occupation, and social class. He finds that only 40 per cent of those living in this limited area were born in the capital, thus confirming Chevalier's conclusion that most of the inhabitants of Paris during the revolutionary era came from the provinces. Jean Claude Perrot has chosen the city of Caen for a case study of passive citizens as enumerated in 1792. He concludes that some 9,099 of a total population of 35,000 belonged to families ineligible to vote, and he indicates, in imaginative graphs and tables, the geographical distribution, the size of their families, and the rent they paid. Michel Vovelle has examined parish registers, fiscal rolls, state documents, and hospital records and has collected a mass of statistical data on births, marriages, and deaths in the city of Chartres from 1760 to 1820. In an impressive array of graphs and tables, he demonstrates that marriages and births zoomed during periods of crisis at the national level and that migrations from rural areas to Chartres during this era provide evidence that the Revolution may have triggered a trend that was the precursor of radical nineteenth-century changes in France's rural-urban population ratio. The series of studies concludes with notes by Reinhard and M. Merlet on special problems relative to the use of birth, marriage, and death records during the revolutionary era.

In their totality the articles represent original research of the highest order. The authors' conclusions are circumspect and cautious, fully cognizant of the limitations of studies based on isolated cases and limited numbers. They have achieved their basic objective: to define problems, indicate sources, and provide a methodology that will stimulate and guide future work in this field.

University of Colorado VINCENT W. BEACH

THE THIRD REPUBLIC OF FRANCE: THE FIRST PHASE, 1871–1894. By *Guy Chapman*. (New York: St Martin's Press. 1962. Pp. xxii, 433. $12.00.)

PROFESSOR Chapman tells us that his projected three-volume study of the Third Republic is directed to "the discovery and dissection of the events in France that led to the defeat and the downfall" of that regime in 1940. He eschews "straightforward narrative history" and adds that in the age of demos (since 1848), "it is impossible to conceive pure political history." The first volume of his work nevertheless comes about as close to being pure political narrative as one can imagine, and the central problem that he has set out to elucidate tends to be swept under the rug. There are occasional chapters on social and economic matters, and in the final pages Chapman does seek to give some meaning to the story that he has told. Still, most of the book is devoted to a blow-by-blow account of parliamentary shadowboxing and is packed with detail about almost forgotten politicians and cabinet crises. At times, indeed, the narrative becomes so congested as to make

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it almost incomprehensible except to the *cognoscenti* (see the second paragraph on p. 233 or the rapid tour of agricultural France in Chapter IV).

Chapman's general thesis is that France's political and economic shortcomings in the late nineteenth century may be traced to two factors: slow population growth, and the revolutionary land settlement. In the absence of pressures that drove other nations to adapt to the modern world, the French turned in upon themselves. "The lethargy of economic and social growth left a vacuum in French political life, which was almost inevitably filled by sterile controversies on forms of government, methods of representation and abstract principles in which the average citizen took no interest." This is certainly a reasonable hypothesis, but it is asserted rather than argued, and it does not take much account of other possible explanations.

Chapman's erudition, his intimate acquaintance with the men and events of the period, cannot be challenged. He provides first-rate accounts of such episodes as *seize mai,* the Boulanger episode, the Panama affair. Like most erudite authors, he has a penchant for unexplained allusions and, even more often, for brusque obiter dicta advanced without the support of much visible evidence, as, for example, in his repeated disparagement of Gambetta ("appalling mismanagement," "died just in time to save his reputation"); his assertion that the government of national defense feared the Paris mob more than it feared the Prussians; his reference to the Franco-Russian alliance as "one of the more spectacular blunders of French politicians"; and his characterization of the 1914 war as "a war undertaken by [France's] ally on behalf of a Balkan state." Sometimes, too, he offers no explanation for such important political developments as the republican takeover of the Senate in 1879 or the desertion of Gambetta in 1882 by half the members of his own parliamentary group.

In his later volumes, perhaps the author will grapple more directly with his central problem. If so, he may yet add some breadth and depth to our understanding of modern France.

Stanford University GORDON WRIGHT

ACTION FRANÇAISE: ROYALISM AND REACTION IN TWENTIETH-CENTURY FRANCE. By *Eugen Weber.* (Stanford, Calif.: Stanford University Press. 1962. Pp. xiv, 594. $10.00.)

IN his preface Weber says that he feels his "presentation is woefully sketchy, no more than the preliminary spadework in a field in which books need to be written where there are chapters now." This may be so, but the reader will discover that it is too-modest an appraisal. Americans have already made many studies of that unattractive but compelling practitioner of literary violence, Charles Maurras, and of his baleful friends and talented associates; they have been distinguished, among the best in any language. But the present work is easily the most thorough we have, a painstaking, carefully documented study that puts the whole subject

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in fresh perspective and illuminates the Third Republic. It is based on the mass of published materials, of course, but its author has ranged far beyond that to seek out the private papers of Maurras and others, police dossiers, unpublished manuscripts, correspondence and interviews with surviving witnesses and actors. He may regret the "official documents and private papers which I sorely lacked," but his audience will be impressed by the volume of materials he has examined and by the extent of the cooperation he obtained in France, which is itself the measure of his competence and industry.

For Weber the *Action Française* movement was a deadly serious social phenomenon, transcending its actual numbers and the circulation of its organs. If the peak of its influence was attained in the decade 1915–1925, its ultimate importance seems now to have been revealed in the fateful years immediately prior to 1939. By that time it had survived condemnation by Rome and was on the way back into the graces of an increasingly Right-wing oriented Church. Like the bourgeois party of established order, the men of *Action Française* detested the regime, but hated the idea of social change still more. As Edward R. Tannenbaum has recently done, Weber concludes that "They did not like the present because it was not sufficiently like their cozy image of the past; they did not like the future, because it might be even worse." But the present study insists much more upon the political and social consequences of these reactionaries. *Action Française* was important in polarizing conservative thought in France and in bringing the majority party of established order to that situation of total paralysis in internal and foreign policy which characterized the nation on the eve of the Second World War—"more deeply and unhappily divided than it had been since 1848." Military defeat and national reconstruction under Vichy provoked no fruitful revamping of Maurrassian thought: both collaboration and resistance were condemned in favor of a doctrinaire cohesiveness that Frenchmen could not know, and the realities of Laval and De Gaulle showed up as mere impotent theory. Until that time *Action Française* had successfully triumphed over its paradoxes in the areas of religion and royalism. But now, "Step by step, by the remorseless logic of the position they had taken, the once incorruptible enemies of the Germans became the servants of the German will." The internal conflicts of hatreds had in effect destroyed the movement. Yet even then the Maurrassian legacy was to be substantial. *Action Française,* says Weber, "left its mark on friends and enemies, and upon France itself, where few men now maintain liberal democracy as a political ideal."

There are fresh pages here on Maurras's relations with the Orleanists, a fascinating reconstruction of the strange death of Philippe Daudet, convincing evidence of the widespread Maurrassian influence within the Church, new information on the events of February 6, 1934, and a valuable survey of the European repercussions of Maurras's thought. It is not extravagant to say that the work is a major contribution to the history of modern France and to the intellectual history of our own times. "Brilliant" is a word that ought perhaps to be used sparingly, but

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it would be difficult to deny it to this superb book. The writing is clear and lively, the viewpoint profoundly informed, detached, and fair. The whole work has style. Like everything the author writes, it is marked by the insights and the dramatic presentation of an imaginative mind. No such work has appeared in France. It does honor to the historiography of modern France. It is one more proof of the excellence that continues to characterize the finest scholarship in America.

University of Toronto JOHN C. CAIRNS

THE ACTION FRANÇAISE: DIE-HARD REACTIONARIES IN TWENTIETH-CENTURY FRANCE. By *Edward R. Tannenbaum*. (New York: John Wiley and Sons. 1962. Pp. viii, 316. $7.50.)

TANNENBAUM undertakes to show how the reactionary groups in twentieth-century France used the theories of Charles Maurras and of other die-hards to restore the old France. He is more concerned with the tactics of these groups than with their ideologies, though he treats the latter adequately for his purpose. The period mainly under consideration is that between the Dreyfus affair, which gave birth to the *Action Française,* and the end of the Vichy regime in the defense of which it died. However, an introductory chapter and an epilogue help the reader to understand how the counterrevolutionary tradition evolved and what happened to it after the *Action Française* expired in 1945.

This is a very valuable book for several reasons. First, the author is to be commended for the skill with which he depicts the relations of the *Action Française* with the pope and the Orleanist pretender, with Catholic groups, and with the more important fascist and nationalist organizations. This is meritorious because it gives the reader a better understanding of the Rightist movement as a whole and of its successes and failures, for one of its weaknesses was the inability of the related groups to unite and cooperate in working toward essentially common goals. It is, of course, true that they were not all royalists, but all of them were authoritarian in their outlook. Second, the author attempts to identify the types of people who were attracted to the various groups and especially to the *Action Française*. This is difficult because organizations such as he is dealing with tend, by their nature, to inflate their claims of support. Their lists of members and their respective occupations, even when checked against secret police records, may be open to question, as the author warns. Even so, it is helpful to discover in a general way the social status of the people who were attracted by these reactionary organizations. Finally, although Tannenbaum makes it very clear that he personally has no brief for the ideas and tactics he is portraying, the impartial manner in which they are presented is impressive.

Although there is some confusion over the reference to all of the reactionaries as one group when it is made abundantly clear that it was faction ridden, this study is an important contribution to the literature on the subject. Every student

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of the history and institutions of twentieth-century France should find reading it a very rewarding experience.

Hendrix College W. C. BUTHMAN

LA FINLANDE DANS LA POLITIQUE EUROPÉENNE EN 1809–1815. By *Päiviö Tommila.* [Studia Historica, Number 3.] (Helsinki: Société Historique de Finlande. 1962. Pp. 478. $4.00.)

THE Napoleonic Wars had important repercussions in the Baltic region, and it is within this context that the position of Finland is to be considered in this period. Thus the present work deals with a much wider subject than its title suggests, in effect, the whole Baltic-Scandinavian situation between Tilsit and Vienna. General histories of the period have given little attention to these matters. The studies of Bernadotte by Sir Dunbar Plunkett Barton and Franklin D. Scott appeared before the publication of several important Scandinavian monographs. The period has, however, received much attention by Scandinavian, particularly Swedish, historians. The enigmatic characters of the main protagonists, Bernadotte and Alexander I, leave ample room for speculation and dispute.

Basing his study on a wide variety of archival sources, Tommila threads his way judiciously through this literature, providing the non-Scandinavian reader not only with the most up-to-date discussion of his subject, but also with a unique introduction to the historiographical disputes that have surrounded it. The author takes exception to a number of interpretations by recent authorities, most notably the late Torvald Höjer. Tommila tends to hold that the main lines of Bernadotte's policy—acceptance of the loss of Finland, cooperation with Russia, and the gaining of Norway in compensation—were largely developed by the time he went to Sweden. The author assigns a much more modest role to Bernadotte and to Sweden generally in the negotiations and alliance with Russia in 1812–1813 than most Swedish historians have done. The consequences of Bernadotte's designs upon the French crown in Swedish foreign policy from 1812 to 1814 are stressed. The pre-eminent role of G. M. Armfelt, who has deserved to be better known outside the Baltic world, in all of the interlocking Baltic and Scandinavian questions receives special attention. The interrelationships between the situations of Finland and Poland with regard to Russia are considered. The variations and changes in Finnish public opinion concerning the new status of Finland are discussed here at length for the first time in a major Western language.

The present study is the third in the Finnish Historical Society's new "Studia Historica," which is publishing "monographs on the history of Finland in its international context" in English, French, and German. Both Tommila and the editors of this welcome new series are to be commended for this valuable contribution to the wider historical world.

University of Alberta H. A. BARTON

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INFÖR UNIONSUPPLÖSNINGEN 1905: KONSULATFRÅGAN. By *Jörgen Weibull.* (Stockholm: P. A. Norstedt & Söners Förlag. 1962. Pp. 449. Kr. 40.)

Most of the writing about the dissolution of the Norwegian-Swedish union has been Norwegian; this treatment helps to redress the balance. It does not argue a Swedish case, but it presents the whole gamut of varied Swedish and Norwegian viewpoints, and it is especially useful in explaining the complications of the Swedish position. Particularly, this volume illuminates the puzzling changes in the policy of the chief Swedish Minister, Erik Gustaf Boström, and the influential roles played by King Oscar and Crown Prince Gustaf. Although it was the Foreign Minister, Alfred Lagerheim, who in 1901 took the initiative in trying to reconcile the Norwegian demands for a separate consular service with the continued existence of the union, Boström was sympathetic. Gradually, however, the rigid stand of the Norwegians and their refusal to back down from their demands for an ultimate separate foreign ministry as well made him skeptical. Meantime King Oscar began to see that agreement on the consular question alone would not preserve the union, that this problem was inseparable from the question of the foreign ministry, and that the foreign ministry must be a union institution. The correspondence between Oscar and Boström helps to explain why, by the autumn of 1904, Boström took a harder line, which he knew the Norwegians could not accept. Crown Prince Gustaf, who had been "more Swedish than the Swedes," at length discovered during his visit to Oslo in March 1905 the depth and universality of Norwegian feeling and knew that the union was finished. He appealed to the Swedish Council of State to take the initiative in dissolving the union. This the Swedes could not bring themselves to do, and the Norwegians were left to direct the course of events. On June 7, 1905, they brought an end to what they called an "unnatural union."

Docent Weibull deliberately excludes from consideration the economic aspects of the conflict and the tensions arising out of the differing social and political philosophies of the two countries (including the important fact that the monarchy had been subordinated to parliamentary authority in Norway while it remained still the dominant political power in Sweden). He holds the account strictly to "the political course of events" and does not even pause to characterize the men involved. These negotiators, like others, were indeed mouthpieces of governments and parties, but they were also men with emotions and with limitations of language and mind. Is not an understanding of negotiators as human beings essential to an understanding of negotiations?

The study is built on documents—protocols of meetings, resolutions, memoirs, notes of telephone conversations—many of them recently released by the "fifty-year rule." About one-third of the volume consists of such documents; others were published in *Scandia* (XXVI [1960], 167–229). An extra dividend is provided by the extensive newspaper discussion, which the author has used skillfully to show the development of partisan opinion in both countries. On the long three years of ne-

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gotiations, this is doubtless the "definitive" account. It is systematic, judicious, and told with dramatic force.

Northwestern University FRANKLIN D. SCOTT

THE RELIGIOUS RENAISSANCE OF THE GERMAN HUMANISTS. By *Lewis W. Spitz.* (Cambridge, Mass.: Harvard University Press. 1963. Pp. viii, 369. $7.75.)

HISTORY cannot be written without generalizations, but generalizations must continually be checked against particulars. This book deals with the religious positions of the German humanists one by one. Since they are compared with each other, the stress in the body of the work is on their individual differences. A concluding chapter assesses their common characteristics. The opening section gives an admirable bibliographical résumé. Had this book been intended for the uninitiated, an introductory chapter would have been helpful on the variegated pattern of ideas on which these men drew: from classical antiquity, medieval Catholicism, and Italian humanism. But such knowledge is assumed, and particular elements from the background are introduced only as they become pertinent to the understanding of individuals.

Agricola, the father of German humanism, blended the *devotia moderna* with Stoicism. He was unaffected by Neoplatonic strains, a devoted son of the medieval Church with none of the tensions of a Petrarch. Wimpfeling was a German Savonarola, an acrid moralist, who applauded Luther at first, but having lived "indecently long" ended with a defense of the Mass. Reuchlin, consumed by the *libido sciendi,* was a mystic who fitted Neoplatonism with cabalistic cosmology and reveled in Pythagorean number symbolism. At first he endorsed Luther, then repudiated him, and at the end became a priest. Celtis was an ebullient, erotic poet, lured by Italian humanism though he hated Italians, intrigued by the glory that was Greece though he lauded the primitive Germans. He rejected transubstantiation, but was addicted to medieval hagiology while swirling amid Neoplatonic *Lichtmetaphysik* and Cusa's *docta ignorantia.* Hutten was anticlerical but not antisacerdotal; antischolastic, but not antireligious. Basically he was a medieval Catholic of the imperialist brand. Mutian, steeped in the Neoplatonic revival, spiritualized religion, decried externalism, and showed leanings to pantheism and universalism. Pirckheimer exhibited Stoic moralism in a Christian frame. The piety of Erasmus was at bottom that of the Brethren of the Common Life tinged with Stoicism. From the Neoplatonists he learned only the distinction of flesh and spirit in the sense of the corporeal versus the incorporeal; hence, his depreciation of all the externals of the Church. Last of all comes Luther. One would have expected Melanchthon. But all along the author is contrasting northern humanism not only with Italian humanism but also with Reformation. Luther was himself a humanist at the point of philological method, and he recognized the validity of

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reason within its own sphere. The great difference was that he was more historical in his thinking than the humanists because he saw man's salvation anchored upon a unique event in history, the incarnation of God in Christ.

By way of summary, Spitz concludes that all the German humanists were Christians. All criticized ecclesiastical abuses. All protested against the dialectical verbiage of late scholasticism. All were synergistic, as was the Catholic Church. They centered on the harmonies of nature, Luther, on the disharmonies. They experienced the *malaise de l'univers*. Luther had a profound sense of personal unworthiness. They did not develop a *Weltanschauung* contrary to that of the Church, but they opened the possibility of absolutizing a culture independent of Christianity. They did not win the masses perhaps because they were themselves divided. One wing emphasized the reasonable, humane, logical, and learned; the other, the mysterious, occult, theosophic, enigmatic, luminous, and ecstatic.

This is an enlightening book about a movement that aspired to be enlightened.

Yale University ROLAND H. BAINTON

VOM "KOLONIALENTHUSIASMUS" ZUR KOLONIALPOLITIK DER MONOPOLE: ZUR DEUTSCHEN KOLONIALPOLITIK UNTER BISMARCK, CAPRIVI, HOHENLOHE. By *Manfred Nussbaum*. [Studien zur Kolonialgeschichte und Geschichte der nationalen und kolonialen Befreiungsbewegung, Number 8.] (Berlin: Akademie-Verlag. 1962. Pp. 167. DM 13.20.)

WESTDEUTSCHER NEOKOLONIALISMUS: UNTERSUCHUNGEN ÜBER DIE WIRTSCHAFTLICHE UND POLITISCHE EXPANSION DES WESTDEUTSCHEN IMPERIALISMUS IN AFRIKA UND ASIEN. Edited by *Heinz Tillmann* and *Werner Kowalski*. [Schriftenreihe des Instituts für Allgemeine Geschichte an der Martin-Luther-Universität Halle-Wittenberg, Number 1.] (Berlin: Rütten & Loening. 1963. Pp. 280. DM 24.)

To reinterpret the history of the creation of the German colonial empire so as to fit it into the Marxist-Leninist scheme of things presents formidable difficulties. The apathy, if not opposition, of many sections of the German business community, expressed politically by the *Freisinnige,* and by a wing of the National Liberals; the composition of the leadership of colonialist pressure groups, in which bureaucrats, military men, and rather dubious adventurers of the stripe of Carl Peters played a larger role than any representatives of monopoly capitalism—these are facts on which all historians of the Bismarckian colonial policy agree, and which Nussbaum is himself forced to accept. He attempts to resolve this difficulty by positing a change in the nature of German colonialism, occurring around the turn of the century. According to Nussbaum, the original acquisition of the colonies in the 1880's preceded the establishment of monopoly capitalism as the directing force of German policy. It was thus motivated not by the search for investment opportunities and monopolistic raw material bases, Leninist criteria for

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"modern imperialism," but primarily by the desire for commodity markets, affecting only a part of the capitalist community. The author is not convincing in explaining how and why this relatively small group was able to dictate the policy of Bismarck, whom he continually calls a *Junker,* that is, not a capitalist at all. It is more than doubtful that the Chancellor believed that colonies would create sufficient new markets for German industry to improve markedly economic conditions and thus to undermine the strength of the Socialist movement.

Nussbaum also fails to make a very good case for his theory of a fundamental change in German colonial policy around 1900. The fact that some of the large banks acquired interests in the colonies is after all far less remarkable than the niggardly amounts so invested. The author's own figures indicate the continuing negligibility of the colonies as a source of raw materials. The opposition to colonialism of the middle-class opposition parties did not decline after 1900, but reached a new intensity from 1905 to 1907. The more energetic colonial policy after the latter date, however, with its emphasis on economic development, was characterized at least as much by public as by private investment activities.

Despite its extensive documentation from the archives of the German Colonial Office—virtually inaccessible to Western scholars—this work adds little factual information about the early years of the German colonial empire.

The Tillmann-Kowalski volume contains five studies on West German "neocolonialism," of which the authors see evidence in economic aid activities in Africa and India, in the insufficiently anti-imperialist tone of West German schoolbooks, and in the German-Israeli compensation agreement. If a view of Israel as a bridgehead of German imperialism should commend itself to any reader, or if it should be felt that phrases such as "infamous Fascist libels" and "the cries and groans of the innocent victims of imperialism" substantiate the Marxist claim to a monopoly of scientific historical scholarship, this book deserves attention.

University of Vermont WOLFE W. SCHMOKEL

THE STRUGGLE FOR GERMANY, 1914–1945. By *Lionel Kochan.* [Edinburgh University Publications. History, Philosophy and Economics, Number 15.] (Edinburgh: Edinburgh University Press; distrib. by Aldine Publishing Company, Chicago. 1963. Pp. viii, 150. $3.95.)

KOCHAN tells us with refreshing candor that he tries to use as much hindsight as possible in writing history and always attempts "to read the past in the light of the present." His most recent essay seeks to explain the reasons for the present condition of a divided Germany. His theme is continuity, and his thesis is that partition was the inevitable consequence of the long struggle between Russia and the West for control of Germany.

From 1917 to the present, Kochan argues, both Communism and democracy have sought to weaken each other by cultivating Germany as a powerful ally. Lenin's program of alliance with "Rightist revanchists" became the basic Soviet

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foreign policy for a decade. Western policy, from Versailles to the occupation of Prague, shows a similar continuity. Britain, in particular, consistently cultivated Germany as the chief bastion against the Bolshevik tide. Thus, Kochan insists, the main purpose and consequence of Locarno were to strengthen Germany and to isolate Russia. This wooing by East and West gave Germany the opportunity to play off one rival against the other and extract favors from both. Such was the policy of Stresemann and such was the program of Von Maltzan, head of the eastern department of the *Auswärtiges Amt* who observed that the justification for Rapallo was that it led to the Dawes report. The Dawes plan and Locarno, in turn, were used by Germany to win concessions from Russia in the form of the Berlin Treaty of 1926.

The advent of Hitler did not interrupt this game of power politics. *Der Führer*'s goals had been sought by Weimar officials. Stresemann, like Hitler, had refused to accept Versailles, and he, too, wanted revision of the Corridor, *Anschluss* with Austria, the return of the Saar, and the protection of all German minorities then "living under a foreign yoke." Von Seeckt prophetically had envisaged a limited war against France and England in order to smash Poland. Such a war, Von Seeckt insisted, would require an alliance with Russia. With the Nazis in power, Great Britain simply continued its policy of cultivating Germany as an ally against Russia. Hitler's aggressions were not allowed to disturb England's policy of concession. Russia's attitude toward the Third *Reich* displays a similar basic continuity. For a time, it is true, Litvinov had tried to isolate Germany by means of collective security, but by 1938 Russia had returned to its more traditional policy of alliance. Early that year Chamberlain rejected Litvinov's effort to summon a conference to deal with the Nazi threat, and in the fall the Munich settlement convinced Russia that it could find no security in a Western alliance. The Kremlin took Molotov's advice and sought to lessen the Nazi threat by associating with it. After World War II, since neither East nor West could achieve its historic desire of becoming the exclusive partner of Germany, partition was the only feasible solution. Thus the Germans, who during the entire interwar period had so sedulously and successfully exploited the rival courtship of East and West, have become literally torn between the two.

Kochan has taken the Biblical injunction seriously: "Seek and ye shall find." He has sought continuity, and he has found it. Such a search, and the deliberate use of hindsight, obviously prevents the author from giving a balanced discussion of interwar diplomacy as it impinged on the German problem. But Kochan has not intended to write such a history. Nor has he sought to make any major contributions to our specific knowledge of the period. He has succeeded in writing a revealing and readable interpretive essay in which the stark clarity of his prose admirably suits his theme of *Realpolitik*.

Williams College ROBERT G. L. WAITE

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IM HAUPTQUARTIER DER DEUTSCHEN WEHRMACHT, 1939–1945: GRUNDLAGEN—FORMEN—GESTALTEN. By *Walter Warlimont.* (Frankfurt am Main: Bernard & Graefe Verlag für Wehrwesen. 1962. Pp. 570. DM 34.)

General Warlimont's book is an account of the activities of the German high command in World War II, with particular emphasis on the subjects of organization, planning, the interrelationships of the principal figures in the high command, and their relationships to Hitler.

Within the Armed Forces High Command (*Oberkommando der Wehrmacht—OKW*), the joint staff headed by Field Marshal Keitel, was the Armed Forces Operations Staff (*Wehrmachtführungsamt* to August 1941, thereafter *Wehrmachtführungsstab*), directed by General Jodl. Warlimont was the second ranking officer of this staff and head of its planning section at the beginning of World War II. From January 1, 1942, he was deputy chief of the Armed Forces Operations Staff, being promoted to lieutenant general on April 1, 1944. On many occasions he acted for Jodl, who was often away at Hitler's personal headquarters. He frequently prepared directives or drafts conveying Hitler's commands to the army, navy, and air force and attended many of Hitler's situation conferences. He is the only person now surviving who was continuously assigned to the Armed Forces Operations Staff from 1938 until he was incapacitated in the autumn of 1944.

In a sense the work is an apologia for the German military leadership in general and for the activities of the Armed Forces Operations Staff in relation to the other German command and planning offices in particular.

The work is extensively documented, and many of the documents are quoted at length. Principal documentary sources cited are the war diaries of the Armed Forces Operations Staff and its subdivisions, the diaries of Generals Halder and Jodl, the *Führer* conferences on naval affairs, fragments of the stenographic records of Hitler's situation conferences, and wartime diaries and memorandums.

Much space is devoted to organizational problems in the high command structure and to the changing relationship of the *OKW* to the high commands of the army and the other services. Subjects of particular interest are the discussions of the value and advisability of the Norwegian campaign, the escape of British forces from Dunkirk, the announcement to the planning section of Hitler's intention to attack Soviet Russia, the description of how headquarters directives were prepared, and the account of the events of July 20, 1944, when Warlimont was rendered unconscious by the bomb explosion at Hitler's headquarters. In September 1944 the effects of this injury forced Warlimont to go on leave, from which he did not return before the end of the war.

Of great interest throughout the book are accounts of Hitler's participation in the planning and conduct of operations. The author considers that it will appear more clearly from his book than from other postwar writings that not only did

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the officers immediately around Hitler have little power to influence him, but that even the field commanders when summoned to headquarters could scarcely escape the compulsion that the *Führer* exercised over them. He believes, to that extent, a vindication of the officers in the *OKW* is justified, but beyond that the judgment of them should be left to history.

Iowa Wesleyan College JAMES S. BEDDIE

GESCHICHTE ÖSTERREICHS: VON DEN ANFÄNGEN BIS ZUR GEGENWART. By *Erich Zöllner*. (Munich: R. Oldenbourg Verlag. 1961. Pp. 672. DM 36.)

THE author of a comprehensive national history is confronted with special problems inherent in the large scope of his undertaking. To what audience should his book be directed? Just how much of the vast body of specialized literature pertaining to his subject should he examine before writing his treatise? Precisely what aspects of his kaleidoscopic topic should he omit or touch lightly upon and which ones should he emphasize? Finally, should the writer of the history of his own country strive to keep all traces of national bias out of his account? The admirable features of the above volume, as well as some of its defects, stem at least in part from the way in which Zöllner, a distinguished Austrian medievalist, has attempted to solve these perplexing problems.

Zöllner has addressed his work both to the professional historian and to the public at large. Perhaps as a consequence, even though many parts of the volume are written in a clear, lucid style, the less well-informed lay reader may easily get bogged down in a plethora of names, dates, and facts. This is especially the case with the sections dealing with intellectual history, in which the author includes so many names that his efforts border on being merely encyclopedic. The professional historian, on the other hand, may still find much of the discussion entirely too brief for his own needs and the volume lacking in special interpretations. This is certainly not because the writer has only a superficial knowledge of those areas of Austrian history which lie outside his own specialized field. Throughout the volume, whether dealing with the medieval period, the early part of the nineteenth century, or the second Austrian Republic, Zöllner demonstrates his fine grasp of his subject and his familiarity with the important literature of the field. His fifty-page bibliography, in fact, is so comprehensive and up to date that it alone might well be worth the cost of the volume.

Perhaps one reason why the author has been impelled to compress so many details into a relatively short space is that he has written about such a broad spectrum of Austrian history. Not only does he deal with political, military, and diplomatic affairs, but he includes special sections on education, religion, literature, music, architecture, and art; population movements; trade; agricultural, industrial, and commercial developments; and economic policy for each period of Austrian history from the High Middle Ages to 1955. These are not integrated into the

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general discussion, but are dealt with separately, era by era. Chronologically speaking, the volume is well proportioned and is not weighted in favor of the author's own specialty. Undue stress, however, has been placed on German political, cultural, and economic developments. When the Magyars, Czechs, Poles, or other nationalities in the monarchy are brought into focus, it is usually mainly to explain or to illuminate developments in Vienna or in the German-speaking areas of the monarchy.

Given the bitter ideological cleavages in Austria during the past half century, Zöllner's history is remarkably free from political bias. The author presents both the good and the bad in the liberal and conservative policies and programs in a fair, unbiased manner. There can be no doubt, however, that the volume has been written from a patriotic, pro-Austrian viewpoint. A reader brought up in a pro-Prussian tradition or one who is sympathetic to the subject nationalities of the former Habsburg monarchy will probably not agree with all the author's conclusions.

Rice University R. John Rath

LE ORIGINI DELL'ORDINAMENTO COMUNALE E PROVINCIALE ITALIANO: STORIA DELLA LEGISLAZIONE PIEMONTESE SUGLI ENTI LOCALI DALLA FINE DELL'ANTICO REGIME AL CHIUDERSI DELL'ETÀ CAVOURIANA (1770–1861). Volume I; Volume II, DOCUMENTI, Parts 1 and 2. By *Adriana Petracchi*. [Pubblicazioni dell'Istituto per la Scienza dell'Amministrazione Pubblica, Studi e testi, Number 1.] (Venice: Neri Pozza Editore. 1962. Pp. 411; 623; 422.)

As Professor Salomone pointed out in the October 1962 issue of this *Review* ("The *Risorgimento* between Ideology and Myth"), Italian historians, since the war, have been re-examining the history of unitary Italy from different ideological viewpoints. Moreover, hitherto neglected aspects of how the newly formed state gained cohesion and stature and how it fashioned its administrative and institutional structure are being studied from original sources. To the growing number of such studies must be added this detailed work on Italy's communal and provincial organization. The author, a graduate in political science, lectures at the Università Cattolica of Milan on political institutions.

Working primarily with sources in the Turin State Archives Dr. Petracchi has traced in her first volume the vicissitudes of local administrations in Piedmont since the eighteenth century. Volume II, Parts 1 and 2, reproduces a rich collection of fifty-one documents on which the text in Volume I is based. Part 1 covers the period 1770–1854 and includes the *Regolamento* of 1775, the various legislative measures promulgated during the Restoration, including excerpts from the careful instructions drafted by the Congress of Vienna to safeguard Genoese local liberties against the absolutist tendencies of the House of Savoy, and concludes with the liberal reforms initiated in 1847–1848. Part 2 traces Rattazzi's policies culminating

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with his 1859 law, Cavour's ideas on local liberties, and the Minghetti proposal for regional organization and administration in 1861.

What emerges is a panoramic presentation of the conflict between two divergent interpretations of the source and distribution of authority in the state. One sees authority emanating from all citizens and widely distributed among them with local liberties and prerogatives preserved and strengthened. The other concentrates authority at the top and reserves all decision-making functions to the administrators of the central government. The trend toward centralization of authority emerges in the eighteenth century when the House of Savoy, like other European monarchies, began to whittle away at remaining feudal privileges and liberties. The reforms of Victor Amadeus II and Charles Emanuel III in the early part of the century tended to concentrate all power in the hands of the monarch. Royal intendants enforced the king's authority throughout the state as the monarchy became absolute. At the same time social changes brought about the slow rise of the middle class and the beginning decline of the traditional Piedmontese military aristocracy. The French Revolution and the Napoleonic legislation had profound repercussions on the local administrations, transferring control of them into the hands of the provincial bourgeoisie. With the Restoration came conflict between the central government and the communal administrations. This conflict, Petracchi maintains, carried over into united Italy. The government imposed strict controls over local expenses. In 1848 a reaction against this trend began as attempts to grant greater local freedom were made in the Piedmontese Parliament.

The next decade saw many discussions on decentralization within the Piedmontese state, small and mountainous. When Lombardy and, later, the rest of Italy were annexed, the issue grew more complicated. The unitarians insisted that decentralization would weaken unity and that the way to create unity out of disunity was to strengthen the power and authority of the central government. The deputies were fearful of losing their political influence over and control of the local clienteles under a system in which provinces and regions would exercise a great deal of autonomy. Minghetti's proposal for a regional reorganization was therefore defeated, and Italy emerged with a centralized state as the Piedmontese *Statuto,* laws, and administration were extended throughout Italy.

This book has both scholarly and timely interest in view of the continuing debate on the issue of regional organization, provided for in the republican constitution, but largely inoperative. Petracchi sets the problem in its historical framework. Her book will prove invaluable for an understanding of this basic and continuing problem in Italian life.

Regis College EMILIANA P. NOETHER

THE MAKING OF THE RUSSIAN NATION. By *Henryk Paszkiewicz.* (London: Darton, Longman and Todd. 1963. Pp. 509. 84*s.*)

THIS book is a sequel to Paszkiewicz' *The Origin of Russia* (1954). The prob-

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lems treated in it are identical, and it has been prompted by a motivation of self-defense. Thus much of the book is devoted to the refutation of reviewers. The author goes, however, beyond this polemical task. To reinforce his scholarly conclusions, he has studied and analyzed considerable additional literature that appeared during the decade following the composition of *The Origin of Russia:* the bibliography following the *Making of the Russian Nation* contains over 2,500 items (added to the almost 2,000 of those consulted by Paszkiewicz in the process of writing *The Origin of Russia*). This immense literature is described clearly and succinctly and is analyzed; from this viewpoint Paszkiewicz' new volume is a very valuable source of knowledge, especially for a reader to whom works in Russian are not available.

Paszkiewicz also proceeds in his new book to strengthen by additional evidence those points which in *The Origin of Russia* drew upon themselves most criticism: the religious meaning of the word *yazyk,* the problem of "The Descent of the Rus'," that of "Old Rus'ian or East Slavonic Nation" (in this connection Paszkiewicz attributes to the term Rus' after the baptism of Vladimir only an ecclesiastical meaning devoid of all ethnic connotation), and, finally, that of the absence of any substantial "Slavonic Colonization of the North-Eastern Lands." The knowledge displayed by Paszkiewicz in adducing evidence is extensive, his skill in handling sources is impressive, and his tenacity in pursuing his demonstration to the finish is remarkable. In spite of these qualities, Paszkiewicz' work fails on several essential points to carry conviction to an objective and careful reader.

This qualification does not concern Paszkiewicz' analysis of "The Descent of the Rus'." Here, the author's argument in favor of the Norse origin of the Rus' is on most solid ground. It is only to be regretted that Paszkiewicz added to this argument an additional reference to his peculiar semantic analysis of the word *yazyk,* for this analysis is the weakest point of his book. This weakness is the price he had to pay for having deliberately rejected the help of specialists in the history of old Russian (or, to use Paszkiewicz' new term, "Rus'ian") language and literature.

What does Paszkiewicz demonstrate in his chapter "Ethnosyazyk"? Only that the term *yazyk,* in the ecclesiastical literature of the old Rus' and in its chronicles, could be occasionally used to define not only the language or the ethnic character of a group, but also its religious character. This is hardly new and is parallel to the use, in medieval Latin terminology, of the words *gens* or *natio.* That, in spite of this, *yazyk* means basically language for the writers of the old Rus' is proven by the very text on which Paszkiewicz based his exclusive interpretation, namely the *Primary Chronicle,* if one only reads the fragments quoted by him in the context of the preamble. The story of the settlement of the Slavs follows in it that of the confusion of tongues (*yazyki*!): the *yazyk slovenesk* appears in the chronicle as one of the seventy-two tongues of divided mankind (not seventy-two religions?).

Paszkiewicz' theory that Suzdal'-Vladimir Rus' was basically a Finnish nation begs two essential questions. First, the language of the liturgy was not old

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Russian (or "Rus'ian"), but the Church Slavonic; how could it lead linguistically to the population speaking "Rus'ian"? Second, how could complete (!) Slavonization occur among the widely scattered and isolated Finnish population, overwhelmingly rural, of the northeastern Rus'?

It is finally impossible to accept Paszkiewicz' contention that the pre-Mongolian Rus' had no other links than those of ecclesiastical hierarchy and common religion. Here again the philological analysis of the term *Ruskaya zemlya* is the weakest of his arguments. It is true that it could be used with a strong religious connotation, but it expressed, nevertheless, a cultural, ethnic, and, to some degree, even political unity (common dynasty and institutional pattern).

University of Washington MARC SZEFTEL

GESCHICHTE DER ORIENTALISCHEN VÖLKER RUSSLANDS BIS 1917: EINE ERGÄNZUNG ZUR OSTSLAWISCHEN GESCHICHTE RUSSLANDS. By *Emanuel Sarkisyanz*. Foreword by *Berthold Spuler*. (Munich: R. Oldenbourg Verlag. 1961. Pp. 422. DM 50.)

NATSII SSSR I RUSSKII VOPROS [The Nations of the USSR and the Russian Question]. By *I. A. Kurganov*. (Frankfurt am Main: Possev Verlag. 1961. Pp. 263.)

IT is not generally realized outside the community of specialists that non-Slavic peoples comprised over one-third (34.4 per cent) of the population of Russia in 1897 and almost one-quarter (23.7 per cent) in 1959. The percentage of non-Russians, if one includes the Ukrainians and Belorussians as minorities, was 56.5 in 1897 and 45.2 in 1959. Yet despite the great upsurge of scholarly interest in Russia in recent years relatively little attention has been devoted to the minority nationalities. These represent a fascinating laboratory of political and sociological problems for both the student of Russian history and of contemporary Soviet society.

Sarkisyanz, already well known for his study of *Russland und der Messianismus des Orients* (1955), sets himself the task of rescuing from obscurity the important non-Slavic sector of the population of Russia which is rarely given the attention it deserves by Russian historians and is more often than not ignored by foreigners. After a brief history of the vast steppe region that stretches from the northern shores of the Black Sea to the Northern Pacific, the author proceeds with a brief account of each of the major peoples from the earliest times to the end of the empire. The treatment is encyclopedic rather than narrative, and it is intended more as a work of reference than as a general interpretation. It is, nevertheless, much more than a mere chronology, for Sarkisyanz has succeeded in incorporating a considerable amount of economic and social data. He does not, however, discuss the more general problems of nationalities, federalism, and separatism which confronted the empire.

Kurganov's work is of a very different type, concerned as it is with the implementation, results, and implications of the policy of assimilation which the Com-

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munist party has been pursuing since the Revolution. Basing his work primarily on the Soviet censuses of 1926, 1939, and 1959, he demonstrates that assimilation has in fact been proceeding steadily as a result of educational policy, urbanization, economic development, and internal migration. Particularly interesting are his figures on migration, which indicate that in the Kazakh and Kirgiz Republics the Russians and other nationalities outnumber the natives by a considerable margin. Indeed, in all but three of the republics the natives constitute less than 80 per cent of the population. Kurganov also demonstrates that the nationalities with a smaller population, including the Armenians, are being assimilated more rapidly than the others. He therefore concludes that Western policies which seek to profit from tensions between Russians and non-Russians in the USSR are based on a mistaken understanding of the nationalities problem.

These two works differ significantly in purpose and method, but together they serve as a valuable introduction to the history and sociology of the formation of a Russian nation from a wide variety of peoples with differing backgrounds.

Princeton University CYRIL E. BLACK

DIE SOWJETISCHE GESCHICHTE DES GROSSEN VATERLÄNDISCHEN KRIEGES, 1941–1945. By *Boris Semjonowitsch Telpuchowski*. Edited by *Andreas Hillgruber* and *Hans-Adolf Jacobsen*. Translated from the Russian by *Robert Freytag-Loringhoven et al.* (Frankfurt am Main: Bernard & Graefe Verlag für Wehrwesen. 1961. Pp. xvi, 97, 576. DM 29.50.)

WITH this translation of Telpuchowski's study, the editors present the West German reader with the current Soviet interpretation of the USSR's role in World War II and at the same time offer an example of how Soviet historians treat recent events.

Telpuchowski begins his history with a review of European diplomacy in the late 1930's, by way of introducing the Nazi-Soviet pact; the Soviet "liberation" of Western Ukraine, Western Belorussia, and the Baltic States; and the Soviet-Finnish War. He then considers the German attack against the Soviet Union and the early Soviet defeats. A large portion of the book is devoted to the subsequent Soviet victories against both the Germans and the Japanese and to Soviet wartime economic achievements. Of far more interest to the American reader is the author's treatment of the wartime alliance between the USSR and the Western Powers. He views this alliance as a temporary stage in an otherwise prolonged struggle between the imperialist powers as a whole and the lone socialist state. Accordingly, Britain, France, and the United States sought to involve Germany and Japan in a war with the USSR in the late 1930's, thereby removing the threat of socialism and of German and Japanese competition for world markets. But the Soviet government, by signing the nonaggression pacts with Germany and Japan, frustrated this plan and instead gained time to improve the country's defenses. Since, however, the contradictions among the imperialist powers were far sharper than the

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contradictions between the imperialist powers and the USSR, the Western Powers found themselves fighting with Russia against the Axis Powers. Yet even during the war, according to the author, the Western Allies, fearing a Soviet victory, gave only minimal aid to the Soviet Union, postponed the second front, and sought a separate peace treaty with Germany. Nevertheless, the Western Allies had to open a second front in order to stem the Soviet advance in Central Europe and then dropped atomic bombs on Hiroshima and Nagasaki in order to end the war against Japan before Soviet armies could liberate all of Asia. The author concludes with a classic *non sequitur* in behalf of peaceful coexistence: "The history of inter-allied relations during the war confirms by means of facts the possibility of the closest collaboration and friendly relations between the Soviet Union and the Western allies. . . ."

The editors, in an introductory chapter to the history, present the different Soviet interpretations of World War II, showing how these have been decreed by the party to reflect its various domestic and foreign policies. Thus, Telpuchowski's treatment of the "Grand Alliance," which is currently in vogue, reflects the party's continuation of Stalin's cold war, albeit behind the new and quite thin façade of Khrushchev's policy of peaceful coexistence. Moreover, Telpuchowski, in blaming Stalin for the early Soviet defeats, is merely acceding to the party's current campaign against the "Stalin cult of personality," while his almost complete silence concerning the wartime activities of Molotov, Malenkov, Kaganovich, and Zhukov is a response to the party's punishment, in 1957, of the "Anti-Party group" and the former Minister of Defense. The editors have also appended additional data to Telpuchowski's history in order, first, to refute his many allegations, for example, that the Soviet victory at Stalingrad brought about Italy's military collapse, that Stalin at Tehran opposed a postwar partition of Germany, and that American agents sought to negotiate a separate peace treaty with Germany at the Soviet Union's expense; and second, to expose, thereby, his tendentious selectivity, and in some instances his falsification, of the available data. Unfortunately, the editors, in addressing themselves to Telpuchowski's many sins of commission, fail to note his many sins of omission, including Churchill's and Roosevelt's warnings to Stalin about the impending German attack against the Soviet Union, the global commitments of the Western Allies, which compelled them to curtail their aid to the Soviet Union, and the Soviet government's own negotiations with German agents in Sweden in 1943.

Still, the history, as a record of events, is not completely without value. The editors, for example, point out that the study contains much heretofore unpublished material, particularly concerning military operations.

Hunter College MICHAEL M. LUTHER

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Near East

THE FIRST OTTOMAN CONSTITUTIONAL PERIOD: A STUDY OF THE MIDHAT CONSTITUTION AND PARLIAMENT. By *Robert Devereux.* [The Johns Hopkins University Studies in Historical and Political Science, Series LXXXI (1963), Number 1.] (Baltimore: Johns Hopkins Press. 1963. Pp. 310. $6.00.)

UNTIL relatively recent years nineteenth-century Ottoman history has been an area neglected by Western scholars. Therefore, works dealing with that period are greeted with some enthusiasm. Devereux's volume deals with the much-neglected Ottoman Constitution and Parliament of 1876 to 1878. His thesis is that the Constitution was presented by its authors in good faith and that the Parliament conducted itself well, on the whole, and had some influence. He supports this thesis well with material gathered from many varied Turkish and Western sources. Even his bibliographic note and bibliography can be read with profit.

In developing his thesis Devereux gives a good brief sketch of the movement for reform in the Ottoman Empire. He then turns to the drafting of the Constitution, the analysis of the Constitution, and the introduction of the constitutional regime. Most of the book is devoted properly to a discussion of the nature and work of the Ottoman Parliament in its two sessions.

The general interpretations and conclusions are not new, but Devereux bases them on more solid research than has been done before. Certainly his conclusion that we should "consider the Midhat institutions as the culmination . . . of all that had gone before" is difficult to deny.

Questions might be raised on a few points. Devereux expresses surprise at the observance of the letter and spirit of the Constitution by the palace and the Porte where financial matters were concerned. Is this surprising in light of Abdul-Hamid II's constant willingness to accept almost any program that would strengthen his empire financially and thus make it more independent of foreign influence? At another point he describes the Ottoman Senate as "superfluous" and a mere imitation of European bicameralism. Is it not possible that the Senate was very useful to the Sultan because it could promote inaction through legislative deadlock and free him from the embarrassment of vetoing bills that he considered undesirable?

These are extremely minor points. The book is a needed and most welcome addition to the literature on the Ottoman Empire in its later stages. The author's style is clear and readable, yet he does not neglect details or fail to cite sources. The book is, to the best of my knowledge, the most comprehensive treatment of the First Constitutional Period.

Wisconsin State College, Oshkosh ROBERT C. DELK

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Africa

A SHORT HISTORY OF NIGERIA. By *Michael Crowder.* (New York: Frederick A. Praeger. 1962. Pp. 307. $5.75.)

THE POLITICAL DEVELOPMENT OF TANGANYIKA. By *J. Clagett Taylor.* (Stanford, Calif.: Stanford University Press. 1963. Pp. vi, 254. $6.00.)

STUDIES IN EAST AFRICAN HISTORY. By *Norman R. Bennett.* [Boston University African Research Studies, Number 4.] (Boston: Boston University Press. 1963. Pp. viii, 93. $3.50.)

THESE three books neatly represent the mixed state of African historiography. In *A Short History of Nigeria,* Crowder, a young Briton recently appointed secretary of the University of Ibadan's African Studies Institute, has compressed the complicated story of Nigeria into a short, balanced narrative that perceptively reflects the modern approach to the study of Africa's past. Nine chapters are devoted to Nigeria before the British conquest, four deal with the colonial phase, and three discuss the rise of nationalism and the achievement of independence. If ancient, medieval, and recent Nigeria is slighted, the history of the nineteenth century is recounted at much greater length in accord with the availability of the results of recent research.

Crowder has used a wide range of secondary material with care and sensitivity. His conversations with historians and anthropologists who were engaged in current research also enabled him to incorporate fresh evidence and revisionist opinion into his narrative. As such, this work must supersede Burn's earlier text as the standard national history. But, despite its over-all quality, his book is not definitive; speculative hypotheses tend to be treated as proven, a few unfortunate inaccuracies exist, and references are omitted (a regrettable decision on the part of the original British publisher). Moreover, Crowder appears to have composed his book with undue haste; in detail the scholarship is at times spotty, the prose style infelicitous. His is a pioneer synthesis that amply demonstrates the need for a full-length, thoroughly documented history of Africa's largest nation.

Unlike Crowder's treatment of Nigeria, Taylor's book is in the manner of the old colonial histories. An inadequate chapter on Tanganyika before 1920 is followed by a discussion of "The People" that utilizes generally discarded concepts and clichés. The third chapter takes the story to 1940, and the remaining six chapters cover the period to independence. In order to do so, Taylor has carefully drawn together information contained in a variety of official reports, books, and periodicals. For the most part the exclusive use of these secondary sources has resulted in an administrative history that only occasionally offers political or economic insights.

Part of the problem is that Taylor, an educational missionary in Southern Rhodesia, appears to have used the secondary evidence uncritically. In the third

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chapter, for example, there are at least four major points where the author has slavishly adopted the official view of British policy contained in government reports, whereas a somewhat broader reading would have cast considerable doubt upon such conclusions. Despite abundant references and a long bibliography, he has demonstrated the danger of writing a monograph from printed sources without any firsthand acquaintance with the subject. A number of important books and articles have evidently not been consulted. In addition, Taylor's writing does not escape being occasionally repetitious and banal. Nonetheless, for want of another, his book will remain an important contribution until Margaret Bates publishes her excellent thesis, "Tanganyika under British Administration."

On its dust jacket, Bennett's book is described as "a step in the march of modern scholarship to recover the historic past of a neglected continent." In fact, it is a series of small steps—a collection of four short essays connected only because each is about an aspect of nineteenth-century East Africa. A sketch of the activities that involved or revolved about Mirambo, an important chief in western Tanganyika, precedes an interesting chronological account of American traders resident in Zanzibar. The founding of and the political role of the Holy Ghost Mission in Zanzibar and Tanganyika form the basis of the third essay. The very slight final note is a preliminary assessment of an agent of the sultan of Zanzibar who briefly governed part of Gogoland. Two appendixes include a nineteenth-century missionary's history of the Nyamwezi and a chart showing the price of ivory in the Zanzibar market from 1826 to 1897. Unfortunately, the book is poorly bound and printed.

Bennett, a member of the Boston University African Studies Program staff, has with evident industry collected the material for these essays in a number of American, European, and African archives and libraries. His footnotes indicate this reading, but they also reflect an unwillingness to use oral data unless written down and an interest in detail rather than in interpretation. As such, these are the essential mines from which others will quarry the stuff of a history of nineteenth-century East Africa.

Harvard University ROBERT I. ROTBERG

RELUCTANT EMPIRE: BRITISH POLICY ON THE SOUTH AFRICAN FRONTIER, 1834–1854. By *John S. Galbraith*. (Berkeley and Los Angeles: University of California Press. 1963. Pp. x, 293. $6.95.)

THIS is a tour de force. Refusing to be enmeshed by minutiae, Galbraith has made a brilliant clarification of a very confused period in South African affairs by looking at them as the imperial government had to view them. This view commits him to the thesis that the ultimate determinant was the imperial factor, and it is well taken: even the dissident trekkers hoped only to escape that factor, not to challenge it.

Viewing its interests in the Cape Colony in the same oceanic manner as its

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predecessors, the British regime found itself involved in an effort to stabilize a frontier that refused to be stabilized, to the frustration and expense of the imperial government. The lobsters' quadrille of apparently inconsistent and contradictory policies danced by Whitehall of advance and retreat, treaties and no treaties, really was a desperate search for a method of implementation of this fundamental policy of frontier stabilization. Considered in this "imperial manner," the facts give no evidence for overwhelming missionary influence nor any "plot" against the Afrikaner people. Missionary influence operated only in terms and to the extent of frontier stabilization, and the one concern of the Colonial Office with the Great Trek was whether frontier stability permitted a border drawn behind the trekkers or demanded one including them. If there was a single determinant in imperial frontier policy during these years, it was the influence of cold experts of the Treasury and the harassed Chancellor of the Exchequer.

In detailing the formulation and application of seemingly contradictory policies, the writer (it is hoped) has finally slain the hoary legend of Lord Glenelg's evil influence because his study clearly shows that the decision to give up the Queen Adelaide Province was taken in full cabinet. The author brings forward the interesting information that the opportunity given D'Urban in this "Glenelg" dispatch to justify the annexation was owing to William IV, who was not just "Silly Billy" after the Reform Act.

I believe that the treatment given D'Urban is too kind; had he seized the opportunity given him in the dispatch, even if it meant merely reiterating his earlier communications, the cabinet might have accepted annexation. His silence only confirmed Whitehall's opinions. On the other hand, the author has made a trenchant analysis of Sir Harry Smith, who sold his "forward" policy to a too-trusting Whitehall, where it is fascinating to view Earl Grey toying with the notion of a sepoy African force in the face of united cabinet opposition to the idea.

In view of the monograph's emphasis on the financial question, there could have been a greater treatment of the financial embarrassment of the Whig government from 1834 to 1841, to the delight of the Tories in domestic politics, the thing of most concern to any cabinet. There could also have been a considerably greater emphasis on the difficulties in British foreign policy in the Near East after 1850, because increasing British commitments there produced the decisions to disclaim any responsibility for the Transvaal region and then to abandon the Orange River sovereignty. These criticisms, however, cannot detract from the strong merits of the study of this first effort by London to solve the "insoluble problem" (as Gladstone put it) of South Africa.

University of Southern California COLIN RHYS LOVELL

LE CONGO BELGE ET LA WELTPOLITIK (1894–1914). By *Jacques Willequet.* [Université Libre de Bruxelles, Travaux de la Faculté de Philosophie et Lettres, Volume XXII.] (Brussels: Presses Universitaires de Bruxelles; Paris: Presses Universitaires de France. 1962. Pp. 499. 33 fr.)

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As a sovereign state created by the Berlin Act of 1885, then as a Belgian colony, and subsequently as a halfway ward of the United Nations, the Congo has been throughout its history an object of international interest and concern. This detailed and sophisticated study examines the Congo problem in German-Belgian relations during the most active period of European imperialism in Africa. The subject was ripe for definitive treatment since both the Belgian and German diplomatic records were available to the author. He has also exploited, at the University of London, the papers of E. D. Morel, moving spirit in the Congo Reform Association. Publications of the German colonial societies and periodical and newspaper sources have been carefully examined and summarized.

The study divides into two principal periods: from 1894 to 1908, when Leopold II's personal rule ended and the Congo became a Belgian colony; and the subsequent period of reorganization and reform under the Belgian Minister of Colonies, Jules Renkin. Leopold's administration was vigorously criticized for its restrictions on trade, the exclusive concessions granted to development companies, and the system of forced labor, which scandalized humanitarians when exposed in the European press. The public image of the Congo Free State under Leopold was quite as unflattering in Germany as in England, but the official position of the government was one of reserve toward British overtures for international action. From the official records the author shows the extent of propaganda countermeasures taken by Leopold through his "Bureau de la presse" and his two principal agents in Germany, Joseph Cartuyvels and Ludwig von Steub.

Abundant evidence shows the German Foreign Ministry to have been consistently cautious and reserved toward colonial propaganda pressures to make the Congo Basin an avowed object of Germany's colonial ambitions. French reversionary rights to the Congo were a factor, as was also the fear of injury to larger German interests by driving Belgium into complete dependence upon France and England. Neither William II, nor German investment bankers, nor maritime interests were ever won to the idea of a great *Mittelafrika* as a major objective of their *Weltpolitik*. It was generally held by statesmen of the leading powers that small or moribund states, such as Belgium and Portugal, lacked the national energy and resources to develop large colonial holdings and that in the course of events these would fall into the hands of the Great Powers. It was considered desirable and foresighted, through economic investment and the acquisition of adjoining territory, to place oneself in a position to claim a just share in such spoils. This did not mean necessarily the pursuit of an immediate annexationist policy. It was along this line that German official and colonial thinking developed between 1908 and 1914. However, the author concludes that "Il faut bien dire que jamais, le Congo belge n'a été pour Berlin un centre principal ou primordial d'attention."

The merits of this carefully wrought work are the significant materials recovered from the German and Belgian archives, the analysis of German publicity and propaganda relating to the Congo problem, the revelations as to Leopold's

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combating of "Congophobia" in Germany, and the objective treatment of the *Mittelafrika* design that was beginning to take shape in Germany by 1914. Careful documentation, a complete bibliography, and a thorough index round out this study of value and distinction.

University of Virginia ORON J. HALE

Asia and the East

CONFUCIAN PERSONALITIES. Edited by *Arthur F. Wright* and *Denis Twitchett*. [Stanford Studies in the Civilizations of Eastern Asia.] (Stanford, Calif.: Stanford University Press. 1962. Pp. x, 411. $8.75.)

A REVOLUTION may negate, but not quite neglect the past tradition. Mainland China last year pursued active discussions on Confucianism, produced some films on the master, and held a meeting at his graveyard. While the events may have caught off balance some specialists with contracts and projects on current problems, they would hardly surprise the academicians who have quietly contributed to a series of five symposia, now completed with this excellent volume.

A word on previous volumes may help evaluation. The first, *Studies in Chinese Thought* (1953), veered toward intellectual history, then on the rise. The second, *Chinese Thought and Institutions* (1957), redressed the balance. The next three conferences wisely focused on Confucianism, the main body of Chinese civilization, so well known, but not sufficiently understood. Both *Confucianism in Action* (1959) and *The Confucian Persuasion* (1960) demonstrated intricate dimensions within the great tradition but not without deviating vistas beyond its confines. This is now vividly reinforced by the final volume with twelve biographical studies spanning across the sixth through the twentieth century. Wright, who organized these collaborative efforts throughout, deserves a well-earned ovation.

Biographical studies are not easy. Twitchett points out in an introductory essay that the dynastic histories and commemorative writings are patternized accounts rather than individualized biographies. Indeed, one may add that scholars in the past, long realizing such limitations, always looked for hints in these records, often known as "little words [impregnated with] great meanings" and implications hidden "within the words and in between the lines." Contemporary scholars should, as Twitchett stresses, rely mainly on the subject's own writings, put him in context, understand his time before they could hope to understand his life. Nor should they forget that behind him was the tremendous weight of China's remarkably continuous tradition. In another essay Wright draws attention to role selection and playing: how many Chinese consciously chose a historical exemplar and conscientiously tried to live out their lives after the image, thus giving "life" to the tradition and at the same time enriching it. It is suggested to

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both editors that inspiration for such roles came not so much from formal biographies, nor from other reading materials, as it did from the "living tradition": the folk stories, tales, songs, plays, popular religions, as well as oral amplifications and encouragement on the part of elders and tutors.

All articles in this symposium are careful and illuminating studies. This short review, barely mentioning the authors in parentheses, attempts only a few summary evaluations. First, most revealing is the variety of personalities that combined both the Confucian and other, sometimes incongruous, qualities. A model "family instruction" written in the sixth century resulted from a Buddhist dilution of the Confucian ideals (Dien). By the late sixteenth century, dilutions and degeneracies were such that a scholar-official, though highly successful, would accept the apathy in government and achieve greatness in art via Taoism (Wu). Mundane and private lives also played their part. Many literary talents during the seventh and eighth centuries were inspired more by the pleasures of life than by Confucianism (Frankel). The idealism of the reform leader in 1898 probably stemmed more from his psychological traits than from lofty philosophies (Howard). Exclusive Confucian theories and nothing else would, in fact, leave a scholar sterile and empty (Levenson).

Second, several contributions effectively revise standard estimations. A twelfth-century founder of Neo-Confucianism, though great in philosophy, was defensive, insecure, and ineffectual when it came to politics (Schirokauer). On the other hand, the "bad last minister" of the next century was not without idealism in introducing land reforms (Franke). Similarly, a dishonorable tenth-century figure who kept high offices under successive regimes with little sense of loyalty or shame was nonetheless an upright gentleman who promoted Confucian values in private life (Wang). Restored to his rightful place in history is the man who persuaded the Mongols to follow Confucianism; the view that traditional accounts overpraised him is in turn discounted (Rachewiltz).

Third, this volume again high-lights the basic conflict between Confucianism and absolutist power. The success and failure of an outstanding eighth-century statesman hinged greatly upon his personal relationship with the emperor (Twitchett). And a weak pretext was all that an emperor needed to execute a great fourteenth-century poet whose real crime was his political talent (Mote). While the state power was never truly Confucian, yet it could not get along without Confucianism. Ambivalently, history marches on.

Finally, in reading these fascinating articles, one feels that the contributions previously made by Chinese and Japanese scholars should have been given greater recognition. For example, the study of a twelfth-century general who became a hero in myth (Wilhelm) owes much to the groundwork laid by several publications on mainland China. The analysis of the last bad minister, previously mentioned, is indebted to the pioneering work by Miyazaki of Kyoto.

Stanford University JAMES T. C. LIU

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THE LADDER OF SUCCESS IN IMPERIAL CHINA: ASPECTS OF SOCIAL MOBILITY, 1368–1911. By *Ping-ti Ho.* [Studies of the East Asian Institute.] (New York: Columbia University Press. 1962. Pp. xviii, 385. $8.00.)

Social mobility in China has been the subject of several highly competent studies, but none of them gets the reader as far beyond the legal and governmental aspects of the topic as this one does. Ho has tackled the question of social mobility in the larger terms of the "opportunity structure," giving careful attention to opportunities in trade and other occupations and in public and private educational institutions. He has also persuasively argued that the legal grouping of people into a hierarchy of social strata did not closely correspond with social realities. He points, for example, to the socially heterogeneous character of certain local elites and literary groups. Those who "labored with their minds" and those who "labored with their physical strength" were not as cleanly separated in culture and function as may be supposed. Some of the author's conclusions coincide with those reached by others. The generally high rate of mobility into and out of civil service observed by E. A. Kracke for the Sung period and, on the basis of a small sample, by Robert Marsh, *The Mandarins,* for the Ch'ing period, has now been impressively confirmed for the Ming and Ch'ing. The author's findings for the Ming period also conform to Kracke's demonstration of the relatively high rates of mobility in the expanding commercial centers in the Sung period. Historians will be interested in the author's carefully presented evidence of a declining rate of mobility from the relatively open conditions in early Ming to the restricted opportunities in late Ch'ing, when an immense increase in population had not been offset by economic growth.

This study complements the meticulous description of the civil examination system in Chang Chung-li, *The Chinese Gentry,* and his biographical data come from sources different from those used by Chang. He has also significantly refined Chang's definition of gentry by excluding from it the *shêng-yüan.* Ideological factors are not slighted, but one hopes that the author will have more to say on this subject in the future. His brief discussion of the possible social consequences of Wang Yang-ming's revolt against the Sung philosophy is especially intriguing. The assertion that "the point of departure in the Confucian social ideology is that all things, including human beings, are by nature unequal" would seem to require qualification in terms of the *Mencius,* which is cited here, and in terms of Sung and later philosophical literature. Was it consistently argued that the manifest differences among men were essential and immutable rather than accidental and mutable? *Ladder of Success* has deepened and refreshed the study of Chinese society; it should be read and reread by Sinologists.

University of Minnesota Romeyn Taylor

THE WESTERN INVASIONS OF THE PACIFIC AND ITS CONTINENTS: A STUDY OF MOVING FRONTIERS AND CHANGING

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LANDSCAPES, 1513–1958. By *A. Grenfell Price.* (New York: Oxford University Press. 1963. Pp. vi, 236. $5.60.)

PRICE is one of a small but most able group of scholars in Australia and New Zealand who have undertaken to approach the study of the Pacific as a single region of human history and geography. His work at the University of Adelaide and the establishment of chairs of demography and of Pacific history at the Australian National University are evidences of this interest. Despite our powerful interests and commitments in the Pacific, there has hardly been comparable activity in the United States, though excellent studies on detailed portions of Pacific history have been done here.

In this particlular work Price continues some of the inquiries previously undertaken in his *White Settlers in the Tropics* and *White Settlers and Native Peoples.* Adopting a very broad definition of the Pacific region, he begins with a very brief sketch of the ethnohistory of the area before the European invasions. American specialists may find some reason to quarrel with his choice of authorities or with some of his conclusions, particularly concerning the indigenous peoples of the Americas. This will merely underscore the writer's point that there is still much need for further research in the field.

Taking up the record of Western invasions of the Pacific Basin, the author examines first the cultural equipment that the Europeans brought with them, stressing the advantages they held over the peoples of the Pacific in maritime technology and in military and political organization. After a brief discussion of the impact of the conquests and colonization upon certain American, Asian, and Pacific Island societies, Price turns to a theme that has long interested him: the distinctions between settler and sojourner types of colonization. In these discussions he points especially to the positive contributions of Western colonialism and attacks with some acerbity some Indian and Indonesian views concerning Dutch rule in New Guinea. There is a distinctly Australian flavor to the attitudes expressed here concerning the past and present roles of Britons and British descended Australasians in the Pacific Basin. These, indeed, are the most fully documented and illustrated portions of the book; this the author acknowledges.

In a chapter entitled "The Moving Frontiers of Disease" Price attempts to suggest the importance of disease as a factor in facilitating the Western invasions and in subsequent demographic changes. "The Moving Frontiers of Animals and Plants" deals particularly with the introduction of exotic plants and livestock into Australia and New Zealand. Here, as elsewhere, the author indicates that great efforts at research and synthesis are needed to present a picture of the Pacific in modern times. It is his purpose to stress that need and encourage others to make those efforts. In conclusion Price returns to his defense of the positive contributions of the West in Asia, America, and the Pacific Islands.

University of Hawaii DONALD D. JOHNSON

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LAND AND LABOUR IN INDIA. By *Daniel* and *Alice Thorner*. (Bombay: Asia Publishing House; distrib. by Taplinger Publishing Company, New York. 1962. Pp. ix, 227. $8.50.)

THE EVOLUTION OF INDIA AND PAKISTAN, 1858 TO 1947: SELECT DOCUMENTS. [Edited] by *C. H. Philips et al.* [Select Documents on the History of India and Pakistan, Volume IV.] (New York: Oxford University Press. 1962. Pp. xxi, 786. $14.40.)

INDIA AND PAKISTAN: A POLITICAL ANALYSIS. By *Hugh Tinker*. (New York: Frederick A. Praeger. 1962. Pp. 228. Cloth $4.50, paper $1.75.)

THE fifteen papers in Daniel and Alice Thorner's book, which were written in India between 1952 and 1960, fall into three groups. The structure of economic power in rural India is examined in the first; the second group is called "Trends" and includes a survey of economic development, 1760–1960; the third is composed of papers criticizing Indian censuses and sample surveys. Although most of the papers are not historical, any historian concerned with agrarian problems, social structure, or census materials may find, as I did, that the Thorners' methods are exceptionally stimulating. They have applied their rigorous critical faculties to a number of sources often used by students of modern India. In each case they have demonstrated that accepted concepts, methods of investigation, categories, or data are inadequate. For example, in the paper entitled " 'De-industrialization' in India, 1881–1931," they challenge the evidence of the censuses that the Indian economy was becoming less industrial and more rural by showing that the occupational categories adopted by the census commissioners were not consistent. Similarly, in "Agrarian Revolution by Census Redefinition," they attempt to prove that the Indian census of 1951 gives a misleading picture of the "agrarian structure" and the progress of land redistribution. According to the census, 67.2 per cent of the agricultural population are "owner-cultivators" or their dependents, while only 2.1 per cent are "non-cultivating owners" or "rent-receivers" and only 18 per cent are cultivating laborers. These small figures for landlords and landless laborers suggest a wider diffusion of economic power in the villages than the Thorners believe exists, and they indicate how the census commissioner has oversimplified complex relationships, used Western categories inappropriate to Indian conditions, and accepted without caution crudely gathered statistics.

In two historical essays, the Thorners attribute much of India's agricultural stagnation to the unbalanced development of the rural economy during the British period. "The new forms of land tenure and legal procedure introduced by the British afforded to the landlords and the providers of credit a set of unprecedented mechanisms for drawing away from the peasants everything but a bare minimum required to keep cultivation going." The radical shift of economic power in favor of the wealthy was not accompanied by significant improvements in productive processes or techniques. The result was arrested development and perhaps a

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decline in per capita income during the last half century or so of British rule.

Since independence, the government of India has introduced unprecedented land reforms. The Thorners believe, however, that the reforms have been inadequate and have not released the productive forces necessary for an agrarian revolution. Ceilings on land holdings have often been ineffective because of "paper partitions" of landed estates; many proprietors still resort to crop sharing and renting to tenants rather than cultivating and modernizing the land themselves.

C. H. Philips, director of the School of Oriental and African Studies, University of London, has edited a volume containing more than four hundred documents covering "the history of the Indian and Pakistani peoples" from 1858 to 1947. It is divided into three sections: the first two are devoted chiefly to constitutional development and nationalist politics, while the third is called "Problems and Policies of the Indo-British Empire: 1858–1914" and contains lengthy sections on official economic, social, and frontier and foreign policy. Apart from nine brief introductory notes, no attempt is made to explain the significance of any document. Nor are there many explanatory footnotes, although there is a glossary of Indian terms.

Philips remarks in his foreword that "in most modern historical writing on India and Pakistan emphasis has usually been placed on British activities in India, often at the expense of the study of Indian development." This volume was supposed to provide a more balanced selection. It succeeds in the political sections in so far as it includes numerous statements by Gokhale, Tilak, Gandhi, Jawaharlal Nehru, Syed Ahmed Khan, and Jinnah.

In selecting Indian politicians to represent Indian political development, however, Philips and his colleagues have been deeply conventional. They have chosen those Indians whose ideas are readily available in English. They have included only brief and not very representative documents about such important minority groups as the terrorists, the Hindu communalists, regionalists, and Communists. There is not a single mention of Aurobindo Ghosh or Subhas Chandra Bose; M. G. Ranade and the Arya Samaj are each mentioned but once. Lord Curzon, on the other hand, appears on forty-two pages.

Despite this limitation and the bias in favor of constitutional and political affairs, each of the twenty-seven subsections in the volume contains a large number of valuable documents not otherwise available. The sections on the managing agency system, land and agricultural problems, the northern frontiers, and the Native States should be especially useful to anyone interested in modern Indian history.

Hugh Tinker's book is a short, lively, and lucid discussion of contemporary political problems in India and Pakistan. The author traces the major political developments in India through the first three general elections and in Pakistan through the first elections under the 1962 Constitution and Ayub Khan's attempts to establish "Basic Democracies" in the countryside. There are excellent chapters on "The Politics of Language and Caste," "The Checks on Government," and

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the efforts being made in the two countries to involve the masses in the political process. The book may be recommended to the nonspecialist as the most perceptive and well-written short introduction to the politics of India and Pakistan. It has a timely appendix on Kashmir and India's border problems.

Northwestern University J. R. McLane

THE BOXER UPRISING: A BACKGROUND STUDY. By *Victor Purcell.* (New York: Cambridge University Press. 1963. Pp. xiv, 348. $8.50.)

This volume concludes that the antiforeign Boxer movement, stemming from the antidynastic secret society tradition, shifted significantly to its prodynastic orientation in October 1899, when the Empress Dowager adopted an "attitude of resolute resistance" to the imperialistic demand of Italy for Sanmen Bay and when the antidynastic Boxer leaders (Chu Hung-teng and others) were defeated at P'ing-yuan, Shantung, on October 18—thus facilitating from both sides the rapprochement between antiforeign elements at court and among the Boxers. The author has surveyed the extensive literature in Chinese, Japanese, and Western languages, including Chinese documents recently published. The results would have made a superb article.

I say this because the whole first half of the book is an ill-digested and sometimes misleading survey of the Ch'ing government and military, Chinese society, the Western impact, imperialist diplomacy, the reform movement, and the missionary question, in which most of the books and articles a serious student should know are quoted, but with many contradictions left unresolved.

This quote-and-run method lets the author tell us, for example, in the same paragraph on page 73 that "both the Chinese Nationalists and the Chinese Marxists have convicted the Western powers of destroying a nascent Chinese capitalism which was emerging from within the country in order to prevent China from competing with them as an equal. On the other hand . . . Mary C. Wright observes: '. . . the British government adopted . . . a basic policy of non-intervention and moderate co-operation; . . . foreign private groups were effectively prevented from unduly exploiting the unstable situation.' " Again on page 104 we are told that K'ang Yu-wei "was essentially a conservative reformer. . . . Even when he boldly produced a kind of work quite new to China, namely a 'Utopia,' he based it on the *Book of Rites.* Nevertheless [as Thompson says] *Ta T'ung Shu* . . . may truly be characterized as a work of original genius."

This having it both ways, producing a kind of impartial obfuscation of one issue after another, seems to come from the author's desire to string together even the most discordant quotations. On pages 71–72 concerning Robert Hart, he first quotes his great admirer, H. B. Morse, at length and then gives equal time to Ch'en Chih (as quoted by the current Peking propagandist Hu Sheng), who says Hart controls the revenues, "employs hundreds of his followers . . . has obstructed the enforcement of customs tariff regulations . . . looks sincere but in reality is a

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blackguard." Purcell himself refers only to Hart's "ambiguous role" and leaves the "general reader," for whom all this is set forth, to his own wild surmises.

This confusion seems partly due to haste. Page 106: Liang Ch'i-ch'ao in the period 1919–1929 had "ceased to be a vital force in Chinese thought and politics . . . worse still, he was now in league with Tuan Ch'i-jui and his Anhwei warlords." This anachronistic libel is footnoted to "J. R. Levenson, *op. cit., passim*." Levenson's verdict, however, in his *Liang Ch'i-ch'ao* (p. 188) is that "from 1918 on" Liang was, of course, "isolated from the warlords." Military terms are likewise garbled: the post-Taiping regional armies built up by "Marquis [*sic*] Tseng Kuo-fan" and others are termed "militia" throughout; the new armies of Chang Chih-tung and Yuan Shih-k'ai in the 1890's are called "to all intents and purposes 'private armies'" (p. 28); the *pao-chia* is a "constabulary."

The second half of this volume, where Purcell's "own research . . . is concentrated," has more solid substance. It surveys rebel sects, Boxer ideas and practices, and the Chinese documentation for 1898–1900 with much interesting detail. But the same rather Taoist tendencies reappear, to quote and move on and to leave issues unresolved. Chapter IX ("'Pro-Dynastic' or 'Anti-Dynastic'?") concludes that "the setback at P'ing-yuan . . . made the Boxers . . . more willing to adopt the 'pro-dynastic' line. Nevertheless, the parent sect from which they had sprung, including, no doubt, many of the supporters of Chu Hung-teng, remained faithful to . . . traditionally anti-dynastic aims." The late G. Nye Steiger's thesis of 1927 is found inadequate, as he himself would have expected after thirty-five years, yet Langer's summary of it in 1935 is quoted as the final judgment from which "there seems to be no reason to dissent."

Nevertheless (to use Purcell's style), despite these strictures, this volume and its assiduous quotation of the literature will now form a useful starting point for further research, both on the new Chinese materials and on British missionary archives sampled in an appendix.

Harvard University J. K. Fairbank

THE ASIAN CENTURY: A HISTORY OF MODERN NATIONALISM IN ASIA. By *Jan Romein* in collaboration with *Jan Erik Romein*. Translated from the German translation by *R. T. Clark*. Foreword by *K. M. Panikkar*. (Berkeley and Los Angeles: University of California Press. 1962. Pp. 448. $7.50.)

This book was first published in Dutch under the title *De Eeuw Van Azië* (1956), then in German as *Das Jahrhundert Asiens* (1958). The present English edition contains an additional final chapter on Asia since the Bandung Conference, for which J. M. Pluvier assisted in the collection and editing of the material, and a chronological survey compiled by Pluvier. A bibliography of about 250 of "the more important books accessible to English speaking readers" is appended.

The senior Romein, professor of theoretical history at the University of Am-

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sterdam, was trained by Johan Huizinga and during the earlier part of his career wrote extensively on various aspects of the methodology of history, including critical essays on the uses of biography, psychology, and terminology in historical studies. His more specialized works include a history of Byzantium, a study of Dutch historiography in the Middle Ages, and, with his wife, A. Romein-Verschoor, a study of *The Heritage of Our* [Dutch] *Culture*.

In the 1930's and 1940's he became increasingly interested in and critical of Western imperialism in Asia, and, after many years of collecting data, and having had "the opportunity to subject his views to the test of direct experience" as a guest professor in Indonesia during 1951–1952, he produced the present work.

While *The Asian Century* is immensely stimulating and perceptive in certain areas and ways, it also contains some serious defects. These defects are not the result of the author's being "frankly anti-colonial," as the publisher's jacket takes pains to explain, and only partly of the curious division of Asia into "inner and outer zones." (By this the author puts Burma, China, the Philippines, India, Indonesia, Japan, Korea, Malaya, and Siam in an "inner zone" and Afghanistan, Egypt, Iraq, Persia, Israel, Jordan, Lebanon, Saudi Arabia, Russian Asia, Syria, and Turkey in an "outer zone" on the basis of population density. Actually the "frankly anti-colonial" bias is carefully controlled, and the zoning scheme is largely forgotten in the course of the narrative, being replaced by eminently reasonable chronological divisions: 1900–1914, 1914–1919, 1919–1941, 1941–1945, 1945–1955, 1955–1960.

The principal problem is unevenness. While the author handles the history of the Middle East, Russian Asia, and Southern Asia very well, he seems to be rather out of his element in China, Japan, and Korea. For example, he says that "Confucianism today has no fewer than 300 million adherents," which certainly gives a strange impression of the whole nature of Confucianism—as though it were a religious sect. He thinks that Korea was "a part of China" until 1895, a vast understatement of the complexity of the Sino-Korean relationship. His analysis of China in the World War I era is muddled by the erroneous assumption that China joined the Allies in 1914 and by uncertain handling of the May Fourth (1919) movement. And the emergence of Mao to the leadership of the Chinese Communists in the 1930's is not well explained, despite the mention of Benjamin Schwartz's excellent study of this in the bibliography.

Japanese names are spelled archaically and occasionally wrongly, for example, Nippon Kogio Ginko, Tokio, Tojohiko Kagawa. The phenomenon of Japanese ultranationalism is oversimplified; for instance, the dual diplomacy present in the Chang Tso-lin and Mukden incidents is left unexplained, with vague expressions like "according to plan" and "Fujiyama [*sic*] began to smoke," obscuring the fact that very sophisticated explanations of Japan's ultranationalist-expansionist course, based on solid documentation, are already in print. Nor is the complexity and dynamism of postwar Japan fully appreciated.

Despite these defects I wish to emphasize that my over-all impression of *The*

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Asian Century is favorable. Romein has made a courageous effort on a very big task. His inclusion of the whole Asian continent in one study brings out hitherto unappreciated comparisons and contrasts. His avoidance of too-sweeping generalizations and his concentration on facts and events are to be commended. And his pithy short-range generalizations usually hit the mark. Especially worthwhile for American readers is his discussion of the course of post-World War II events where a non-, but I think not anti-, American viewpoint should occasion some critical second thoughts on such subjects as oil nationalization in Iran, events in Lebanon, Syria, Jordan, and Iraq and their relation to Nasser's Egypt, the roles of Sukarno in Indonesia and Ayub Khan in Pakistan, and the approaches of India and Red China to basic subsistence needs.

University of Pennsylvania HILARY CONROY

JAPANS RUSSLANDPOLITIK VON 1939 BIS 1941. By *Hubertus Lupke*. [Schriften des Instituts für Asienkunde in Hamburg, Number 10.] (Frankfurt am Main: Alfred Metzner Verlag. 1962. Pp. viii, 189. DM 24.60.)

A PRAGMATIC hypothesis in analyzing Japan's foreign relations has been that Japanese leaders have tried to prevent Japan's being entrapped in a nutcracker of simultaneous Russian and United States hostility. Lupke's carefully documented monograph depicts how Japan worked to solve its problems with the Soviet Union, 1939–1941, to free Japan's armed forces for conflict in the Pacific.

The policy followed by the Kwantung army from 1931 to 1940 had led to the brink of war with the Soviet Union. There had been Russo-Japanese clashes on the Amur River in 1937, a Japanese defeat in the border battle at Changkufeng in July 1938, and a major Japanese disaster at the Battle of Nomonhan, May–August 1939. Lupke gives this background and then investigates the measures that the Japanese government undertook, in particular, to settle the immediate issues related to the northern frontier and, in general, to seek the elimination of the possibility of a general war with the Soviet Union.

In the first instance, the author covers the settlement of the Nomonhan incident from the establishment of a joint commission to fix the Manchukuo-Mongolia border through the diplomatic solution in which Japan acquiesced to the Soviet Union's position. The Japanese government faced a yearly crisis in the conference that fixed the terms by which that country could operate fisheries in Russian waters in the North Pacific. Lupke gives the terms of the fifth *modus vivendi* and the fishing agreements of 1940–1941; he also covers the Russo-Japanese problems of oil and coal concessions in northern Sakhalin through mid-1941.

In the second instance, he analyzes Japan's policy of seeking a general diplomatic settlement with the Soviet Union from Togo's and Tatekawa's proposals to Matsuoka's surprise conclusion of a neutrality pact in April 1941. He concludes with an examination of Japan's necessity to keep the Soviet Union neutral after the outbreak of World War II in the Pacific.

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This paperback volume includes nineteen appended documents and a bibliography. Lupke utilizes Russian-, Japanese-, German-, and English-language sources. The monograph adds a sound, well-written account to the scanty literature of an important section of pre-World War II diplomatic history. Represented by this kind of scholarship, the *Instituts für Asienkunde* may be watched with anticipation.

University of Oregon PAUL S. DULL

BURMA'S FOREIGN POLICY: A STUDY IN NEUTRALISM. By *William C. Johnstone.* (Cambridge, Mass.: Harvard University Press. 1963. Pp. ix, 339. $7.50.)

CEYLON: THE PATH TO INDEPENDENCE. By *Sir Charles Jeffries.* (New York: Frederick A. Praeger. 1963. Pp. x, 148. $4.50.)

JOHNSTONE's study of the background and development of Burma's foreign policy reflects his thorough acquaintance with contemporary Burma and is based on a series of eighteen Burma Research Project Papers prepared by the Rangoon-Hopkins Center at Rangoon University. A not entirely satisfactory introductory chapter is followed by chapters carrying the detailed chronological narrative from 1948 to 1962. Two others cover Burma's relations with China and its participation in the United Nations, and these are followed by two final chapters of critical analysis and evaluation. Four appendixes list the basic project papers and include the full texts of the Sino-Burman border treaty of 1960, the socialistic manifesto issued by Burma's Revolutionary Council in April 1962, and a perceptive evaluation of U Nu as a personality and statesman.

A basic theme is the impact of Marxism on postwar Burma and the increasing deference paid by Rangoon since 1953 to Communist China. Marxist ideas provided originally a useful weapon to facilitate the achievement of Burma's political and economic independence along with a short-cut method of state-directed industrial development. The moderately pro-Western orientation prevailing during the rebellions of 1948–1952, including acceptance of American economic assistance gave way in the spring of 1953 to an accommodation with Red China. This was due to Burman dissatisfaction with the results of American technical assistance and to resentment over the dangerous Kuomintang refugee problem, for which Washington refused to accept responsibility. Another contributing factor was the ineffectiveness of United Nations (American) intervention in Korea (1950–1953), supported at the outset by Burma, which dashed Rangoon's hopes of finding security through association with the UN. China was strong and near at hand, and endemic rebellion made Burma highly vulnerable. The emerging neutralist policy included friendship for everyone, acceptance of aid from any quarter if accorded without strings, and alignment with neither of the world power blocs. Communist powers encouraged the trend by negotiating economic credits and barter agreements and by Khrushchev's promised gift of a hospital, a techni-

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cal institute, and a sports arena. Since the Communist rebellion in Burma had already ended, Peking by 1954 was urgently concerned to prevent Burma's adherence to the American sponsored SEATO Pact. Peking eventually exploited the long-delayed Burma boundary settlement of 1960–1961 to extort a pledge from Burma not to associate itself with any hostile military alignment and to continue active cultural and economic intercourse. Burma's vulnerability was aggravated by its failure to develop a viable economy, a stable political system, and an articulate sense of national unity and purpose. Friendly relations with China became an end in itself, limiting Burma's freedom to decide such issues as aggressions against Laos and India on their merits. As history, the treatment can be criticized for minor inaccuracies relevant to the prewar period and as a labored and repetitive effort. One could also argue that the serious peril of domestic disintegration that Burma faces would be aggravated by any failure to maintain friendly relations with China and that common sense required therefore a neutralist policy. Johnstone's informed argument nevertheless commands serious consideration by all students of Southeast Asian politics.

Jeffries presents from Colonial Office records an apologetic account of British trusteeship over Ceylon from its beginnings as a crown colony in 1802 to its attainment of complete independence within the Commonwealth in 1948. The author is at his best in the 1940's, where he writes from firsthand knowledge of the compelling reasons for the inexorable trend toward independence, despite admittedly valid reservations, and of the personalities of good will on both sides who were responsible for a peaceful resolution of the problem. An occasional passage reflects the author's emotional involvement. An abbreviated epilogue describes some of the difficulties encountered by the emerging state. Dependence on official British sources limits his consideration of the substance of economic and communal realities that have constituted the essence of the island's political problems. Genuine historical perception would seem to demand a deeper treatment and a broader orientation of the situation following World Wars I and II considered in the context of contemporaneous developments in neighboring India, Burma, and Pakistan.

Ohio University JOHN F. CADY

Americas

INDEPENDENT HISTORICAL SOCIETIES: AN ENQUIRY INTO THEIR RESEARCH AND PUBLICATION FUNCTIONS AND THEIR FINANCIAL FUTURE. By *Walter Muir Whitehill.* (Boston: Boston Athenæum; distrib. by Harvard University Press, Cambridge, Mass. 1962. Pp. xviii, 593. $12.50.)

THIS volume constitutes the report of Whitehill's study of "the research and publication functions and the financial future of the independent historical societies."

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Supported by a grant from the Council on Library Resources, Inc., the author, who is director and librarian of the Boston Athenæum, traveled through three-fourths of the states between October 1959 and June 1960, visiting historical societies and as many other institutions of allied interest as his time permitted. He supplemented information derived from personal observation with that obtained from conversation and correspondence with heads of organizations and from published reports. He did not limit himself to studying the research, publications, and finances of the independent historical societies, but looked into "related activities in many parts of the country," including the work of the publicly supported state historical societies, local historical societies, various types of historical associations, state archival agencies, university libraries, historical museums, art museums, and even certain types of commercial organizations that have been developed to exploit the public's interest in history.

The chapters devoted to the independent historical societies contain able and at times brilliant discussions of the history and present activities of these notable institutions which have rendered significant service to scholarship by collecting, preserving, and publishing important source materials of American history. Individual chapters are devoted to the Massachusetts Historical Society, the New-York Historical Society, the American Antiquarian Society, the Historical Society of Pennsylvania, and the Virginia Historical Society; the smaller societies are treated according to regional groupings. Although no two societies are quite alike, most of them have limited their activities to the service of scholarship and have eschewed the popularization of history. Few, if any, have financial resources equal to their needs. Whitehill recommends that they continue to limit their activities and seek additional financial support from private rather than governmental sources. Although he overemphasizes the perils of public support, he does make a good case for the independent historical society as a "reliable and suitable" recipient of funds from private individuals and foundations.

Discussion of the independent historical societies occupies only about half of the book; the remainder is devoted to comments on the "related activities" that the author looked into as he traveled around the country. These comments are presented to provide "a background for the situation of the independent historical societies, . . . although without claim to completeness or exhaustive treatment." The comments most certainly are incomplete—there is no discussion, for example, of the Pennsylvania Historical and Museum Commission, and the Detroit Historical Museum is mentioned only incidentally and condescendingly; the large, publicly supported state historical societies generally are given inadequate, biased treatment, particularly in the analysis of their activities in the area of popular education and of the obligations and opportunities inherent in the nature of their support. Perhaps the most serious omission is the absence, throughout the work, of an appreciation of the relationship of popularization to the effective use of history.

There are, as Whitehill indicates, two general types of historical societies in

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the United States: the independent society, which is primarily concerned with the advancement of learning and which reaches the public only through the work of those who use its resources; and the publicly supported society, which is concerned with both the advancement of learning and its wide dissemination. Both types, he admits, have their place. It follows then that the activities of both types have their place. If this be true, little is to be gained in trying to exalt the one through the denigration of the other.

Whitehill's stated purpose was to study the independent historical societies. He would have produced a much better book had he confined himself to that purpose.

University of Nebraska JAMES C. OLSON

CYCLES OF CONQUEST: THE IMPACT OF SPAIN, MEXICO, AND THE UNITED STATES ON THE INDIANS OF THE SOUTHWEST, 1533–1960. By *Edward H. Spicer*. (Tucson: University of Arizona Press. 1962. Pp. xii, 609. $12.50.)

HISTORIANS in the United States should perhaps be ashamed that an anthropologist has undertaken a task that probably should have been handled by historians long ago, that is, to attempt a general history of Indian-European relations in the Southwest and to trace the impact of the intruders upon the native populations during the course of four hundred years. This represents a rather ambitious effort for one volume and perhaps is why no historian has made an attempt at such a synthesis. Nevertheless, Spicer was not frightened by the immensity of the challenge, and, as a matter of fact, he enlarged the size of his task with his concept of the "Southwest" as a region extending from Jalisco to Utah and California to Texas. Actually, however, he excludes many native groups from his study because of an alleged lack of evidence and concentrates upon the major tribes of Chihuahua, Sonora, Arizona, and New Mexico.

Spicer writes well, and he has presented us with a challenging set of generalizations, interpretations, and analyses relating to interethnic confrontations. He is to be complimented for his objectivity, his application of anthropological concepts to historical data, and his bold effort at creating a synthesis where none has previously existed. On the other hand, the work suffers from certain weaknesses. Although Spicer carried out his research in 1955–1956, *Cycles of Conquest* was not published until 1961–1962, and during this period a great number of very pertinent works have appeared. The author's failure to use them has weakened his study. This would not be such a serious liability if *Cycles of Conquest* were based upon a thorough use of the extensive archival collections available in southwestern history, but Spicer failed to use a single archival source or government document. At least his bibliography fails to reveal reference to any such materials, and since his work lacks footnotes, one must assume that the bibliography accurately represents his major sources.

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Spicer's study rests upon a moderately extensive, but by no means exhaustive, use of anthropological and historical secondary sources coupled with a use of some published documents. It is something of a disappointment to note, however, that he failed to utilize the valuable writings of Arricivita, Obregón, Morfi, Zarate Salmerón, John R. Bartlett, Tello, Garcés, Arlegui, Vetancurt, Mota Padilla, Arregui, Venegas, Baltasar, Tamarón (for Nueva Vizcaya), Vélez de Escalante, Núñez Cabeza de Vaca, and Velasco, to name but a few. Furthermore, the important documents published in the collections of Hackett (*Historical Documents . . .*), Pacheco and Cárdenas (*Documentos para la Historia de México*), Cuevas, Maas, Hammond, and Rey, Winship, Bolton (*Spanish Exploration in the Southwest* and *Anza's California Expedition*), and Thomas (*After Coronado* and *The Plains Indians and New Mexico*) are not cited. Many well-known secondary books are also absent from the bibliography.

Cycles of Conquest is a valuable work, if only because it represents a notable effort at historical synthesis by an anthropologist. It will inevitably suffer, however, from the fact that Spicer is unaware of a great many significant events in southwestern history (as along the Colorado River between 1783 and 1848). His contribution must be viewed as a tentative, exploratory effort rather than as a "definitive" work.

San Fernando Valley State College JACK D. FORBES

DENIAL OF EMPIRE: THE UNITED STATES AND ITS DEPENDENCIES. By *Whitney T. Perkins.* (Leyden: A. W. Sythoff. 1962. Pp. 381. Fl. 35.)

THIS ambitious book is the fullest study so far of American policy toward dependent areas from the Northwest Territory to Okinawa. The author traces underlying principles and their practical application, first and briefly to localities that could be assimilated to the original thirteen states by a process of "cellular expansion." He moves on to the "dilemma of power" created when the United States emerged from comfortable hemispherism to responsibility for far-flung lands widely divergent among themselves and from the American tutelary authority. He finds "a mystique of freedom" as "the central theme which gives unity and uniqueness to the American experience with dependencies and which also . . . characterizes the American search for a principle to sustain foreign policy commitments."

The work combines able exposition of underlying principles with workmanlike condensation of a vast body of monographic, governmental, and popular printed sources into a single narrative. The double approach, however, involves the author in the perennial dilemma of whether to draw the forest large or the trees in detail. The diligent reader will find the forest, but he must mind the branches.

As a take-off for examining the treatment accorded the noncontiguous and

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overseas possessions acquired more or less fortuitously since 1867, an introductory chapter traces the continental experience in using the Northwest Ordinance as a school for statehood. Alaska, Hawaii, Puerto Rico, and the Virgin Islands are closest geographically but vastly diverse in population and problems. The jump to the Philippines, the Pacific Islands of Samoa, Guam, and Micronesia, and, finally, to the purlieu of Japan in Okinawa completes the survey and multiplies the difficulties.

The narrative reveals with admirable clarity and in great detail the problems arising in trying to apply the continental principle of progress from dependence to equality in diversely peopled extracontinental areas where neither local mores nor the facts of economic life are conducive to the development of the American way. The result has been a wide disparity in policies from place to place, with little evidence of consistency or forethought, but withal a somewhat vague and too often offhanded pattern of good will. A considerable range of status developed, with only Hawaii and Alaska attaining the original goal of statehood, and with the remaining dependencies ranging from independence (the Philippines) through commonwealth and citizenship (Puerto Rico) to almost complete subjection (Okinawa). The moral, of course, is that the uneven result counsels the future application of increasing talent and attention to the needs of the remaining dependencies.

A reviewer should not criticize the book the author did not choose to write. Perkins deliberately chose to combine narrative with analysis and evaluation of American policy; there can be no quarrel with this, for one must rest upon the other. It is, however, a far cry from the discussion of broadly imperial affairs to the mention, however casual, of official efforts to improve Samoan copra production by "organizing a required weekly period of beetle-seeking." Too, a reading directed at matters of substance disclosed about fifty mechanical errors which more careful proofing would at least have minimized. Such matters aside, the volume fills a need and will be highly useful to all seeking either facts or principles in the American overseas experience.

Rutgers University L. Ethan Ellis

THE DEADLOCK OF DEMOCRACY: FOUR-PARTY POLITICS IN AMERICA. By *James MacGregor Burns*. (Englewood Cliffs, N. J.: Prentice-Hall. 1963. Pp. xi, 388. $5.95.)

Burns, author of *Congress on Trial* and *Roosevelt: The Lion and the Fox* among other books, is an able historian as well as an able political scientist. He is concerned, in this current work, over public boredom with politics and the failure of political leadership to come to grips with both the life of urban man and the new ventures in foreign policy.

Basically, he contends, our governmental system was designed for deadlock and inaction. Central to James Madison's thinking at the Constitutional Con-

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vention was his aim to stop people from turning easily to government for help. Madison's ultimate checks and balances were political. "The ambitions of Presidents and Senators and Representatives and judges were bound to collide because each was responsible to separate constituencies . . . ," Burns observes. But between 1801 and 1809, Jefferson established a pattern different from the Madison system. He built a national party directly into the electorate, exerted vigorous leadership over the congressional wing of his party, and carried out majority rule despite the checks and balances in the Madison plan.

Burns then devotes more than half of his provocative book to a historical analysis of the evolution of political parties, the fluctuations in executive leadership, the supremacy for much of the time of fragmented government, and the manner in which the presidential and the congressional wings of the two major parties have contended for control of the federal government. Extremely well-read in the literature of American political history, Burns incorporates a most useful bibliography. His analysis suggests many areas needing reappraisal. And he urges historians to devote more study to those members of Congress who thwarted Presidents and how they entrenched the power of the congressional wing of their party.

In the closing chapters of the book, the author is concerned with the continuing fragmentation of politics into two presidential and two congressional parties. There have been too much acceptance, he remarks, of Madison's system and a neglect of the Jeffersonian concept of strong leadership, majority rule, party responsibility, and competitive elections.

National control over the electoral machinery for national offices, the opening up of one-party enclaves that are at the heart of the two congressional parties, curbing the seniority system, the filibuster, and the malapportionment of congressional districts are among the changes necessary, he contends, for the growth of two nationally responsible, competitive parties. Each presidential party faces the task of converting its congressional party into a wing "exerting a proper, but not controlling or crippling hold on party policy."

Much of what Burns suggests can be done without amending the Constitution. Entrenched leaders of the congressional parties and local leaders who flourish with the noncompetitive one-party system of so many states will hardly rush, however, to end their own power. While acknowledging the difficulties, Burns concludes: "The cure for democracy is leadership—responsible, committed, effective, and exuberant leadership. The man and the party who take the lead in modernizing our political system, in establishing a majority party able to govern and a minority party able to oppose, will have helped put an end to the dangerous cycle of drift and deadlock in our national affairs."

University of Chicago WALTER JOHNSON

FORGE OF DEMOCRACY: THE HOUSE OF REPRESENTATIVES. By *Neil MacNeil*. (New York: David McKay Company. 1963. Pp. xi, 496. $6.75.)

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THE House of Representatives may be viewed from various angles. It may be seen from the viewpoint of the former congressman, as Alexander wrote about it in his procedural history of the House in 1916; or by the college professor, as Bates saw it in 1936 in his legislative history of Congress; or by the political scientist as I analyzed it for the political science fraternity in my condensed, functional history of the House in 1961; or by the journalist from the vantage point of the press gallery, as MacNeil has now done in his popular, personalized, anecdotal history in 1963.

Each of the above works was written with a different emphasis, by a different kind of writer, for a different audience. MacNeil's history has been written by a perceptive journalist primarily for the general reader. It is a study in depth of the House by a warm partisan of that body. His stated purpose has been "to portray the House itself as a living political institution . . . to show the House, in its many facets, as a vital part of the federal government, reflecting the immense complexity and variety of American life." MacNeil's unique contribution is to be found in his vivid, play-by-play account of the dramatic struggle for control of the House Rules Committee in January 1961 and in the many amusing anecdotes, dramatic episodes, and colorful illustrations, culled by diligent research from the memoirs of former legislators, with which he embellishes his story. It is also to be found in his description of the causes of bicameral conflicts with the Senate and his detailed account of legislative-executive relations during the first Kennedy Congress.

For the lay reader *Forge of Democracy* is the most rewarding of all the books in the growing literature on the American Congress. Although rambling and repetitive in coverage, verbose and pedestrian in style, and defensive and uncritical in tone, MacNeil's history is rich and comprehensive in content. In sixteen discursive chapters the author describes the origin of the House, its inner life and psychology, the evolution of the House rules, and the historic rivalry for power between House, Senate, and President. The powers of modern Speakers are traced and explained, the priesthood and party machinery of the House are depicted, the backgrounds and constituent relationships of the "rank and file" are described, and some aspects of the committee system are sketched. Succeeding chapters deal with the House as grand inquest of the nation, lobbying before Congress, expansion of the President's role in the legislative process, the struggle between internal power blocs, the changing character of House debate, and the various tactics used to obtain passage or defeat of administration measures.

MacNeil concludes that the House is the stronger body and that, early and late, it has been "the representative of the people, the depositary of their power, and the image of their wisdom."

At a time when Congress has been under harsh attack, it is well to have a book appear that defends the House and all its works. The skeleton of the first chamber takes on flesh and becomes a living reality under MacNeil's graphic touch. To a greater extent than any previous writer he has succeeded in making

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the colorful, boisterous House come to life. The reader finds no evaluation, however, of current proposals for reform and few, if any, expressions of the author's political philosophy or personal judgments.

This book is a readable restatement of the known facts of House history and practice, and an absorbing narrative of the exciting events of recent years observed at firsthand by the chief congressional correspondent for *Time* magazine.

Library of Congress GEORGE B. GALLOWAY

THE PAPERS OF BENJAMIN FRANKLIN. Volume VI, APRIL 1, 1755, THROUGH SEPTEMBER 30, 1756. Edited by *Leonard W. Labaree et al.* [Sponsored by the American Philosophical Society and Yale University.] (New Haven, Conn.: Yale University Press. 1963. Pp. xxix, 581. $10.00.)

DURING the eighteen months covered by this volume Franklin was concerned almost entirely with matters internal to Pennsylvania. He performed a sort of tour de force in finding and engaging wagons and teamsters for Braddock's expedition. After the disaster, when the whole colony was aroused by the danger of Indian and French attack on the exposed frontier, Franklin, both as a member of the Assembly and as a man of action on the frontier, played an important part in frontier defense. In the latter role, he received a commission to command the militia for Northampton County and organized the building of a string of forts along the frontier. As a member of the Assembly, he played a very important part in the effort to appropriate money for frontier defense. He was instrumental in the passage of the famous Militia Bill, and he served temporarily as a colonel in the voluntary militia raised under the bill before it was disallowed. He kept up his scientific activity, but he worked at it only sporadically; it even appears that his interest in science flagged a little. He retained the job of postmaster and continued publishing *New Richard Improved*. Franklin's most vital interest, however, was now in politics. For he became deeply involved in the Assembly's dispute with the proprietor's governors, Robert Hunter Morris and William Denny, over the question of taxing the proprietary states in the province.

In many ways this dispute was symptomatic of the sort of struggle for power that was going on in the royal colonies between the faction representing the increasingly powerful assemblies and the principle of governmental responsibility to the voters, and that representing the principle of government by instruction, or the prerogative. In Pennsylvania, however, the struggle was complicated by the facts that government was vested in a private individual and that the property involved belonged to a private family and was not the property of the crown. The Assembly argued, quite plausibly, that since the proprietary's lands in Pennsylvania were private property they ought to be taxed as any other private property, especially in this case and at this time since the money raised was to be used for the protection of that very same land. Thomas Penn, the proprietor, argued, through his governor, that the taxation of proprietary lands was contrary to the

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English form of government, since for the Assembly to tax the proprietary's lands would be much the same as taxing the lands of the crown because the proprietary occupied an analogous position in Pennsylvania.

When the proprietary's governors refused to sign appropriation bills containing the clause for taxing the proprietary estates and then went on to suggest how such bills might be rendered more acceptable to the proprietary by certain amendments, the Assembly, apparently using Franklin as its penman, took the high ground of the right of Englishmen to tax themselves and rejected the suggestion that the governor or the proprietary might amend money bills.

Here was clearly a struggle for power between Assembly and proprietary, but the taxation issue was symptomatic of something much deeper. Franklin, after some vacillation and efforts to please both sides, came out flatly on the Assembly side. The editors of the *Papers* are satisfied that much of the Assembly's correspondence with the governor was actually penned by Franklin. It certainly seems so, and although the Assembly's arguments for its position were formulated by "committees," it is probable that the political ideas in the Assembly's arguments were Franklin's own. If so, Franklin here shows himself to be a philosopher of colonial autonomy and of the responsibility of the governor to the Assembly.

In its essence, however, this struggle for power between the Assembly and the proprietary is to be seen also as a conflict between the new, modern idea that government is responsible to the people, on the one side, and the old feudal concept of government as being above the people and not subject to control by them. Here, too, there is a similarity between the struggle in Pennsylvania and that in the royal provinces: did the Penn family have essentially the same rights in Pennsylvania that the crown had, or claimed to have, in the royal provinces?

And, if it did, did those rights, under William Penn's original charter, partake of the nature of the crown's "prerogative" in those colonies? We do not have here the full arguments used by the proprietary to support his cause (it would have been of great value to have them calendared in the footnotes); what we do have indicates that the proprietor thought of his authority in these terms. Penn's charter was already far short of granting the proprietary the palatine powers conceded in the Maryland charter to Lord Baltimore, yet even in Penn's charter the basic principle of government was still fundamentally feudal. That charter, indeed, made Pennsylvania the last feudal outpost in Anglo-America, although the nature of the "Royal prerogative" implemented in the instructions of the royal governors was also, and similarly, feudal. Franklin, with the Assembly, represented the general trend in Anglo-America away from those feudal concepts toward the Lockean and eventually republican principle of complete and unrestricted responsibility of government to the body politic.

The march of history is clearly visible through these *Papers,* and Franklin is marching in the midst.

University of Washington MAX SAVELLE

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DEMOCRACY IN THE CONNECTICUT FRONTIER TOWN OF KENT. By *Charles S. Grant.* [Columbia Studies in the Social Sciences, Number 601.] (New York: Columbia University Press. 1961. Pp. xii, 227. $5.00.)

On its face, this book promises little. Its subject is a single obscure town in western Connecticut during a brief period in the eighteenth century. It is replete with tabularized data on characters whom history has justly ignored. It is wooden in structure and argumentative in tone. The writing is often uncommonly bad, even for this generation of historians, who have abandoned the literacy test as a qualification for admission to practice. Thus seventy people die of smallpox, of whom "twenty-seven were adult males with the remainder women and children." (What else could the remainder have been?) Or witness this marvelously mixed metaphor (pp. 75–76): "These writs and sheriffs' responses represent a dangerous pitfall for the historian who stumbles onto them and fails to realize that their bark was worse than their bite." Or again (p. 99): "In 1777 the town was literally flooded with members of the third generation coming of age." (One visualizes the poor creatures lining up at the town kiln, to be melted down on their twenty-first birthday.)

Yet for all that, it is a serious, important, and often profound book. It explores, in great depth and with meticulous care, the "frontier process" in the town of Kent. Upon analyzing this process in a highly informed and eminently sensible way, the author concludes that the postulations of F. J. Turner and a long list of his distinguished followers have no validity in this particular town, however plausible they might seem after superficial, "broad rather than deep" studies of other places. The town was not settled by an underprivileged class migrating westward; it was not exploited by an eastern capitalist class; it was not a hotbed of eighteenth-century Populism; it was not the scene of a local class struggle; its citizens were not deprived of participation in government; it was not dominated by a squirearchy that "kept the lowly in their places." Nor, on the other hand, does the author find there the kind of middle-class, peace-and-harmony democracy that Robert Brown found in Massachusetts.

In a nutshell, what did happen is this. The original proprietors came from the eastern as well as the southwestern part of the colony. The vast majority were actual settlers. They could scarcely have been poor or even "common," for they paid £150 to £200 each for their shares. Some of them sold parts of their lands at a profit to settlers of similar backgrounds (what people did for a living in Connecticut was sell land to one another). All thrived, and a goodly measure of social and political harmony prevailed. The trouble was, they had huge families, and their sons had huge families. What happened during and after the third generation is explicable largely in terms of the "pressure of an increasing population on the available supply of local land." In its earlier phases the town was democratic, but conservative in religion and politics; it grew increasingly restive later. Though the author does not explicitly draw it, the inference is clear: that it was

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not life on the raw, wide-open frontier that broke down and reshaped old values and institutions, but life in relatively mature, crowded places.

Grant refrains from projecting his findings beyond the town, but in a prudently worded preface he conveys his belief that Kent was not exceptional. I agree. The tragic death of this young scholar (of cancer, not long after the book was published) deprived us of a historian of great promise—and recalls the circumstances that cut short the careers of two other brilliant young students of the period, Isaac Harrell and Joseph Charles.

Brown University FORREST MCDONALD

NICHOLAS BIDDLE: NATIONALIST AND PUBLIC BANKER, 1786–1844. By *Thomas Payne Govan*. (Chicago: University of Chicago Press. 1959. Pp. xii, 428. $7.50.)

FOR a decade prior to Jackson's removal of the government deposits in 1833, the second Bank of the United States was probably the most effective central bank in the world, and Nicholas Biddle, its president, the most knowledgeable banker. The bank provided a stable and uniform currency. It handled smoothly, and with no dislocation of the market, large financial transactions for the government, in spite of well-meaning bumbling by the Treasury. It managed for both the government and private business the intricate details of international exchange. It played a major role in the tremendous economic expansion of the 1820's. In every demand made upon it, the bank served faithfully and well the business community, the government, and the country at large. Yet this same bank, during the administration of Andrew Jackson, was damned, victimized, and ultimately destroyed.

Govan explores step by inevitable step the worsening relations between the bank and the administration. His conclusions are much the same as those of Catterall early in the present century and more recently of Bray Hammond, but they are developed in greater detail and with a special cogency derived from the biographical approach. Biddle was a man of intelligence, industry, imagination, and a touch of genius, whose self-confidence was as boundless as his faith in America. He had already tried his hand with some success at diplomacy, literature, law, and politics before President Monroe appointed him in 1819, at the age of thirty-three, to be one of the five government directors of the bank. By 1823, when he became the institution's third president, he was master of his craft. His directors thereafter gave him a free hand, and so did the government until Jackson stormed onto the scene.

Jackson never understood what the bank did; he simply distrusted it, and his successive Secretaries of the Treasury were almost as ill-informed. Tact and patience might have dispelled much of this official ignorance, but Biddle, domineering and combative, was short on both. He enjoyed the enormous power he wielded, and when it was threatened by politics he responded in kind, using every means at his command, including money, to "educate" politicians, editors, and

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voters. Jackson's veto of the bank's recharter in 1832, the withdrawal of the government funds and the resulting financial stringency, the rechartering of the bank as a Pennsylvania corporation and its failure constitute a familiar sequence, told most frequently since the Democratic resurgence of the 1930's from Jackson's viewpoint. Here is the same story as Biddle saw it, including the panic of 1837, the recovery, and the subsequent recession with all its consequences for himself and for the bank he continued to dominate long after he gave up its active management. It is a welcome and a convincing antidote.

Despite a persistent tendency to assume more knowledge of banking and finance than the average reader possesses, Govan has done an admirable job; so much so that one can only regret the more his decision to confine himself to Biddle's public career. More in the way of personal detail would have made the "public banker" a more understandable and perhaps a more sympathetic figure.

Washington, D. C. CHARLES M. WILTSE

THE PAPERS OF JOHN C. CALHOUN. Volume II, 1817–1818. Edited by *W. Edwin Hemphill.* (Columbia: University of South Carolina Press for the South Caroliniana Society. 1963. Pp. xciv, 513. $10.00.)

VOLUME II of *The Papers of John C. Calhoun* will be welcomed by scholars interested in War Department policies, operational routines of the first Monroe administration, and other military and political aspects of the Era of Good Feelings. In details of editorial craftsmanship and in general and specific knowledge of his subject, Hemphill convincingly demonstrates the suitability of his selection as the late Robert Lee Meriwether's successor. Appreciation should be expressed not only for certain raw materials of history but also for many interpretive comments, offered in the twenty-page preface and in the sixty-four-page introduction.

The only major criticism of the accomplishment is by no means exclusively adverse. Indeed, in the main, I am sympathetic. But it will not suffice to say "Well done!" when a vitally important editorial decision invites discussion for future scholarly and administrative reference. Like editors of similar works, Hemphill found himself confronted by a mass of departmental, interdepartmental, and related correspondence. What should be done with a pension application, a letter seeking appointment as second lieutenant, or a claim for the value of a lost sword? Hemphill's usual procedure is to print a reference and an abstract. Sound enough in each case, it would seem at first glance. Yet doubts come to mind when one realizes that hundreds of comparable items crowd these pages, many having little or nothing to do directly with Calhoun.

When letters from and to Calhoun are patently more significant, they either appear in full or are accorded a longer summary. But the facts remain that there are proportionately few documents of this quality and that the volume covers only eight months of Calhoun's life. If the rest of Calhoun's War Department service

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is to be represented on the scale of eight months per volume, then approximately eight additional volumes will be required to reach March 1825—a quarter century before Calhoun's death. If there are to be no similar introductions, possibly we may think in terms of only six or seven volumes. Even on that basis, the original estimate of fifteen volumes for the entire set will fall considerably short, particularly if Calhoun's congressional speeches and related activities, his private letters, State Department correspondence, and miscellaneous papers are allotted suitable space.

In my opinion numerous routine papers should merely have been listed under appropriate topics. The overriding question is not "How many volumes are the Calhoun Papers worth?" but "Are the standards the right ones for this case?" And I answer it in the negative. At the same time, I acknowledge the array of complications affecting anyone's judgment of so challenging a matter. The problem of the relative value of various categories of private and public papers, or even synopses, is one that the most ingenious experts will long find difficult to solve, especially as they deal increasingly with twentieth-century and later records.

University of Kentucky HOLMAN HAMILTON

AMA: VOICE OF AMERICAN MEDICINE. By *James G. Burrow.* (Baltimore: Johns Hopkins Press. 1963. Pp. xii, 430. $7.50.)

PROBABLY no profession has developed so effective a political lobby nor provoked so much controversy in recent years as that represented by the American Medical Association. Hence Burrow's fine study of this organization comes at a good time. The AMA came into existence in 1846 when medical education was at its nadir, when licensure laws were almost nonexistent, when public quarrels within the ranks were the order of the day, and when the public attitude toward medicine varied from humorous contempt to outright hostility. During its early years, the AMA fought desperately to raise the professional level of practice through improving medical education, developing a code of ethics, and promoting the public health movement.

By 1900 the main goals had been achieved, and the AMA and its affiliated organizations, through the enactment of licensure laws, had succeeded in gaining effective control over the medical profession. Having created a strong pressure group, the profession turned its guns on proprietary drugs and quack medicine and here performed its most effective public service. Meanwhile it fought for a federal health department and for the establishment of state and local health boards.

Up to this point, there is no question that the interests of organized medicine and that of the public coincided, but the post-World War I years, as Burrow shows, are another matter. Whereas formerly the AMA had reported impartially on the governmental health services of Great Britain and other countries in its various publications, after 1920 it bitterly denounced what it termed socialized

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medicine. Even volunteer prepaid medical groups came under indictment, as the association proclaimed that private medical practice and the fee system had given America the best medical care in the world. Henceforth, almost without exception, the AMA was to fight against every measure designed to improve the health of the lower economic groups. In the face of overwhelming evidence demonstrating that millions of Americans had little if any medical care, the profession consistently refused to acknowledge the existence of any real need.

The depth of the depression temporarily silenced the AMA, and it took a moderate position on the Social Security Act. As economic conditions improved and efforts were made to include health and medical provisions in the social security system, the AMA arose in outrage and must receive much of the credit—or discredit—for keeping the role of the federal and state governments in this area to a minimum. As voluntary health insurance programs grew and the threat of compulsory health insurance increased, the association reluctantly began to accept voluntary prepaid medical care, but insisted on safeguards to protect the fee system.

The extension of the British medical program after World War II goaded the AMA into further denunciations of the system and into deliberate misstatements about the reaction of the British public and medical profession. Blindly disregarding the fact that all British political parties espouse the existing health service and that the British Medical Association itself has expressed general satisfaction, the AMA continued to give so distorted a picture of English medicine that even the British Medical Association remonstrated against the misrepresentation.

Burrow has done a remarkably thorough job of research, and his history of the AMA is both accurate and perceptive. He has drawn up a judicious balance sheet, crediting the association for such things as its fight against proprietary drugs and its efforts to impose efficiency upon the Veterans' Administration Hospitals, while at the same time showing its lack of social conscience in the twentieth century and its resort to tactics that are at least questionable.

University of Pittsburgh JOHN DUFFY

MINING FRONTIERS OF THE FAR WEST, 1848–1880. By *Rodman Wilson Paul.* [Histories of the American Frontier.] (New York: Holt, Rinehart and Winston. 1963. Pp. xix, 236. $6.00.)

THE BONANZA WEST: THE STORY OF THE WESTERN MINING RUSHES, 1848–1900. By *William S. Greever.* (Norman: University of Oklahoma Press. 1963. Pp. xiv, 430. $5.95.)

DESPITE an extensive literature on various aspects of the subject, until recently no scholar has undertaken a serious, comprehensive analysis of the western mining frontier as an integral part of the settlement process. Now, simultaneously, two new studies have appeared. One, by Rodman Paul, is interpretive, highly

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selective of detail, and covers the years from 1848 to 1880; the other, by William Greever, is essentially descriptive and extends the narrative to 1900.

As indicated in the foreword by Ray Billington, Paul's *Mining Frontiers of the Far West* is the first of eighteen projected volumes in the "Histories of the American Frontier" series. As such, it is a superb beginning and should be at once gratifying and stimulating to the other authors of the series. With a fresh, penetrating approach, Paul strips away the glamour and views the mining frontiers (there were many, rather than one) not only in their bearing on national development, but also in terms of their interrelationship with each other. He is concerned with the significant and enduring aspects of the mineral West, not with the froth, and stresses especially the role of capitalists and entrepreneurs who developed the mines and the mills and who forged the transportation links so vital to isolated mining camps. Above all, he emphasizes the technological advances, which by the end of the century had brought American mining machinery and mining experts into great demand throughout the world.

Paul divides the history of the mineral West into four distinct periods. The first, 1848–1858, was the era of golden California—an era of trial-and-error apprenticeship, with accent upon simple placer techniques using a large labor input. The second, 1858–1868, brought an extension of placer mining into other parts of the Far West, the maturing and stabilizing of hydraulic and quartz mining in California, the beginnings of corporate large-scale, deep-level operations on the Comstock Lode, and a frustrating exposure to complex ore formations, which often decreased in richness with depth in Nevada's Great Basin. A third era, 1869–1879, belonged mainly to the fabulous Comstock Lode, where engineering advances hastened the exploitive processes and rampant speculation often hindered them. Elsewhere, except for California and Utah, the western mining frontier fell into decay or failed to prosper in the absence of capital, improved transportation, or new methods of handling refractory ores. The year 1879 opened another era, one in which Colorado replaced the Comstock Lode in pre-eminence, thanks to the discovery of rich silver-lead ores and the adaptation of European techniques to reduce them. Soon Utah, the northern Rockies, and ultimately the Southwest would open vast new deposits, their growing emphasis on base metals indicating that western mining was being integrated into the national economy.

In *The Bonanza West,* Greever seeks to present the story of the major American mining rushes in "a single compact, authoritative, and readable synthesis." In a treatment broader and more detailed than Paul's, he ranges from the discovery at Sutter's Mill in 1848 through the metallic West—Nevada, Colorado, Idaho, Montana, the Black Hills—closing with the spectacular rush to the Klondike half a century later. Greever is concerned with virtually all facets of life on the mineral frontier—social, political, cultural, and economic—and is particularly impressed with the similarities he finds. His organization on a geographical basis sometimes sacrifices continuity and makes for repetition, while, in attempting to reach a wider reading public, Greever is inclined to focus too long on the colorful

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but less consequential features of his story. Vigilante justice looms large, for example, and even Calamity Jane, Deadwood Dick, and the Custer "Massacre" are paraded.

By comparison, Paul's writing is crisper, more sophisticated; his research is more extensive, and his thoughtful essay on sources makes his bibliography more valuable; his illustrations are better captioned; his proofreading more thorough. This is not to belittle Greever's work: as intended, it is an excellent synthesis, but adds little that is new. By contrast, Paul's volume brings with it a richness of ideas and interpretations that will undoubtedly make it the more enduring of the two.

University of Illinois CLARK C. SPENCE

ANTISLAVERY AND DISUNION, 1858–1861: STUDIES IN THE RHETORIC OF COMPROMISE AND CONFLICT. Edited by *J. Jeffery Auer*. [Prepared under the auspices of the Speech Association of America.] (New York: Harper and Row. 1963. Pp. xii, 427. $6.00.)

THIS book is similar in organization and purpose to three earlier volumes sponsored by the Speech Association of America: *A History and Criticism of American Public Address* (1943, 1955), edited by William Norwood Brigance, with the collaboration of Marie Kathryn Hochmuth on the third volume. Here again, the chapters are self-contained monographs which combine, in varying proportions, rhetorical criticism with historical narration and analysis. But in this case the editor has severely limited the scope of his book in order to achieve greater intensiveness and unity. The first essay deals with a debate on slavery between William G. Brownlow and Abram Pryne at Philadelphia in September 1858; the last one is an examination of Lincoln's first inaugural address; the other twenty-one fall chronologically in between.

Although a few historians have contributed to the work, most of the authors are professors of speech, or the equivalent, in colleges and universities from coast to coast. About one-third of them offer studies of individual oratorical efforts by sectional spokesmen like William H. Seward and Robert Barnwell Rhett. Several others concern themselves with series of speeches, such as the New England tour of Jefferson Davis in 1858 and the invasion of the South by Stephen A. Douglas in 1860. Many of the essays, and perhaps the most useful ones as a class, analyze the rhetoric of significant groups and meetings. Among the subjects covered are the Republican and Democratic national conventions, the annual meeting of the Pennsylvania Anti-Slavery Society, the general conference of the Methodist Church, the slavery debate in American colleges, the deliberations of the Senate's Committee of Thirteen, and the Washington Peace Conference.

The chapters vary in length from six to twenty-nine pages, and their quality is also somewhat uneven. The reader must struggle through too much unilluminating detail, especially outlines and summaries of speeches. Yet some of the essays are unquestionably excellent, and the general level of scholarship is high. To

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gather so many respectable pieces of writing upon such a restricted theme was an unusual editorial achievement. And why did the editor select 1858 as his starting point? Because by then, he maintains, "most of the critical arguments of past decades on antislavery and disunion had begun to crystallize." Thereafter they were largely "stereotyped" and "irreconcilable." In short, the sectional debate was already about exhausted by 1858, and the oratory examined in this book was for the most part the rhetoric of justification and utter commitment.

Stanford University DON E. FEHRENBACHER

THE CENTENNIAL HISTORY OF THE CIVIL WAR. Volume II, TERRIBLE SWIFT SWORD. By *Bruce Catton. E. B. Long,* Director of Research. (Garden City, N. Y.: Doubleday and Company. 1963. Pp. xi, 559. $7.50.)

THIS middle segment of the strife of 1861–1865 stretches from the morning after First Bull Run in July 1861 to the eve of Fredericksburg in November 1862. The theme of the volume is that in this approximate year and a half the nature of the conflict was transformed and redefined. The naval and military actions of the fall and winter of 1861–1862 had brought the Union forces to the verge of victory; the boundaries of the Confederacy were proving too vast for its armies to defend, and it then seemed that the Union had only to strike in force where it would, and win. But unfortunately McClellan, the commander in chief of the armies, failed to grasp his opportunity to deliver the final blow and literally threw success away.

Had victory been achieved in the spring of 1862, the Union might have been reconstructed again on the old pattern, but when Lee and McClellan destroyed that chance, Lincoln was impelled to undertake revolution. He resorted to emancipation and began his discouraging search for someone who could do what McClellan failed to do, for someone who could use the Army of the Potomac for something more than a parade army, a search that Lee was not to make any easier. It must now be an all-out war involving the destruction of the southern power and way of life, particularly slavery. For according to Catton, "this war, which could not conceivably have occurred if slavery had not existed, was bound to become a war about slavery if it went on long enough." The fearful irony is that McClellan, who so strongly desired a reconstructed Union and the *status quo,* made his great objective impossible because of his incapacity.

With his usual literary mastery, Catton explains once again the military events of these months and, despite the fact that he has done it in some fashion or other three times before, makes them appear fresh and absorbing and even more understandable. He also deals with diplomacy and with trade between the warring sections, still interdependent; for the first time in his writing he gives extensive consideration to the Confederacy. His description and interpretation of the Army of the Potomac are brilliant, his demolition of McClellan devastating, and one might expect final, save for the fact that McClellan's disciples seem able to furnish him with nine lives. Fortunately for the country the same infirmities that prevented

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McClellan's victory likewise stopped him from taking his army and setting up as a dictator when he probably could have done so. We must therefore be grateful that here too he hesitated and that the antiwar Democrats were equally ineffective. More than ever Lincoln's capacity shines forth in this new revelation of his ability to overcome the seemingly insuperable obstacles he faced.

We may look forward to the final volume or volumes of this notable history with even more eager anticipation. Not only is the climax of the struggle to be presented, but it is hoped that Catton will turn his attention again to the meaning of it all. His frame of reference so far, to me at least, appears to be too narrow. When he declares so unequivocally that the war "could not conceivably have occurred if slavery had not existed," he limits his consideration unduly. The findings of regional and behavioral science in such matters as ecology, nutrition, epidemiology, group dynamics, and the like would indicate that the size and consequential ecological variety of this huge country decreed that when its people undertook self-government there were bound to be social variations that would rouse rivalry and conflicting ambitions as the Republic grew in population. The fact that accident placed control initially in the hands of those dwelling in a region that was destined to be outstripped by a more versatile and dynamic segment of the great society made inevitable a conflict for power which, even had there been no slavery, might have led to disastrous consequences, even to war. There is a tendency for great polities to fragmentize, and the United States need not have been an exception.

University of Pennsylvania ROY F. NICHOLS

JUSTICE OLIVER WENDELL HOLMES: THE PROVING YEARS, 1870–1882. By *Mark DeWolfe Howe*. (Cambridge, Mass.: Belknap Press of Harvard University Press. 1963. Pp. ix, 295. $5.00.)

BECAUSE this volume, like its predecessor (*The Shaping Years*), is "the account of a young man's progress towards intellectual accomplishment," the author's task has been primarily that of interpreting the published writings of Holmes as a legal scholar. He has pursued the somewhat "dusty" pathway that Holmes traversed in making himself a master of legal lore. Even marriage to Fanny Dixwell did not deflect Holmes from his relentless ambition. (That the marriage was childless may have been because of his choice.) From 1869 to 1873 he was engaged in editing the twelfth edition of Kent's *Commentaries*. He was also prolific as an editor of the *American Law Review*. He delivered lectures on constitutional law and jurisprudence in Harvard College. Then in 1880 he delivered the Lowell Lectures, which resulted in his epoch-making treatise *The Common Law*, published in 1881. Appointed a professor at Harvard Law School in 1882, he accepted appointment to the Massachusetts Supreme Judicial Court in December of that year, much to the displeasure of Dean James B. Thayer, although Holmes had stipulated with President Eliot that "If a judgeship should be offered me I

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should not wish to feel bound in honor not to consider it, although I do not know that I should take it and although my present acceptance will diminish the chance of such an offer and is for that reason against the advice of many of my friends."

Howe's illuminating analysis of Holmes's legal writings during this period is enlivened by some anecdotes reported by Mrs. Holmes in her diaries of trips abroad in the summer of 1874 and 1882. But central emphasis is rightly given to detailed analysis of *The Common Law,* which claimed all of Holmes's energies between 1875 and 1881 that were not devoted to professional practice. By this time Holmes had come to reject the austere analytical jurisprudence of Austin and the logical *elegantia juris* of Langdell. He recognized that "The life of the law has not been logic: it has been experience as it embodies the story of a nation's development through many centuries, the law finds its philosophy not in self-consistency, which it must always fail in so long as it continues to grow, but in history and the nature of human needs." Holmes had come increasingly to believe that "a valid jurisprudence, whether for the scholar or for the practitioner, must take far more serious account of the materials of history than had the common-law lawyers of the eighteenth and nineteenth centuries. He followed the historian's path, however, not for the purpose of increasing understanding of the past but to the end of comprehending the character of legal institutions."

Perhaps through his associations with Henry Adams, Holmes turned to the writings of German historians who had investigated the background of English legal institutions. In his efforts "to discover Teutonic rather than Roman origins for the institutions and principles of the common law, one may discover a romanticism of spirit. In any case, one can find in *The Common Law* reflections of nearly every facet of a complex mind and temperament." Yet perhaps "his greatest gifts and most ardent tastes were for clarifying *aperçus,* rather than for systematic thought." Though Ames could write "No one, I am sure, values your work in the field of legal history more than I do," it was perhaps a perceptive insight into his own limitations as well as a Puritan sense of duty that impelled Holmes to abandon an academic career for the burdens of judicial office.

Pittsburgh, Pennsylvania EDWARD DUMBAULD

JOSEPHUS DANIELS SAYS. . . : AN EDITOR'S POLITICAL ODYSSEY FROM BRYAN TO WILSON AND F.D.R., 1894–1913. By *Joseph L. Morrison.* (Chapel Hill: University of North Carolina Press. 1962. Pp. x, 339. $7.50.)

JOSEPHUS Daniels managed to compress several significant careers into his long and busy lifetime (1862–1948). For more than half a century he was a crusading North Carolina editor and a prominent leader of the progressive wing of the Democratic party. In addition, he served eight important years as Wilson's Secretary of the Navy, and nearly nine years as US ambassador to Mexico, 1933–1941, where he helped give the Good Neighbor policy direction and meaning. Daniels

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always considered himself first and foremost a journalist, however, and devoted his chief efforts toward making his newspaper, the Raleigh *News and Observer*, an influential force in North Carolina and the upper South. Notwithstanding, Daniels' career as a provincial editor has drawn less attention than his national service. Joseph L. Morrison, an associate professor of journalism at the University of North Carolina, has now sought to redress the balance with a competent study of Daniels' early journalistic career.

Morrison's book covers the first fifty years of Daniels' life, concentrating on the period after 1894 when he borrowed money to purchase the moribund *News and Observer* and gradually made it an editorial, political, and financial success. His story ends in 1913 when Daniels accepted President Wilson's offer to become, as Daniels liked to say, the "Managing Editor of the Navy." Morrison's research is thorough and his treatment judicious. Recognizing that Daniels' own papers for this period are skimpy and incomplete, he has made effective use of collateral manuscript collections and the files of the *News and Observer* and other North Carolina newspapers. If at points he has relied largely and sometimes uncritically on Daniels' memoirs written long after the event, it must be recognized that this is an important source for this portion of his life (though hardly an accurate record of conversations some forty years earlier).

By examining Daniels' editorial stand on key issues, Morrison shows both his growth as a southern progressive and his increasing involvement in Democratic politics. Morrison's focus is largely on North Carolina matters, and his discussion of such national issues as imperialism, Cannonism, and the Ballinger-Pinchot fight is cursory and oversimplified. His chapter on Daniels and progressivism might well have explored more fully Daniels' ties to progressives outside the South and his growing concern for national issues. The statement that Daniels belonged to only two organizations, the Democratic party and the Methodist Church, and served both full time, is scarcely true even in a rhetorical sense. Aside from his membership in several publishers' groups, he was an active supporter of a number of organizations, such as the Anti-Imperialist League and the Anti-Saloon League, that flourished in the progressive era. But these are minor criticisms. Morrison has written a useful and perceptive book, which does not need the promotional appeal of its misleading subtitle: *An Editor's Political Odyssey from Bryan to Wilson and F.D.R., 1894–1913*. There is, correctly, little of Wilson and only the most strained reference to FDR in these pages. As a matter of fact, so little did Daniels know FDR when he appointed him Assistant Secretary of the Navy in 1913 that he referred to him in his diary as "Mr. Frederick D. Roosevelt."

University of Wisconsin E. David Cronon

THE NORRISTOWN STUDY: AN EXPERIMENT IN INTERDISCIPLINARY RESEARCH TRAINING. By *Sidney Goldstein et al.* Foreword by *Thomas C. Cochran.* (Philadelphia: University of Pennsylvania Press. 1961. Pp. xxi, 366. $7.50.)

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It is the fashion to extol interdisciplinary training and to deplore its lack, but only rarely are graduate students in history or the other social sciences given the opportunity even to take courses in other disciplines, much less receive formal interdisciplinary training. One exception was the Seminar on Technological Change and Social Adjustment offered jointly by the departments of history and sociology at the University of Pennsylvania for three years beginning in 1951. From this seminar, which included faculty members and, ultimately, twenty-nine students from the departments of history, sociology, anthropology, psychiatry, and American civilization, there had emerged by 1959 some eleven Ph.D. dissertations and a number of published papers. This book is a progress report on the experiment and a summary of some of the research findings.

Norristown, an independent borough about twenty miles north of Philadelphia on the Schuylkill River, was chosen for study. Between 1900 and 1950, the period under consideration, Norristown maintained its independent identity while its population expanded from 22,265 to 38,126, and industrial firms grew, died, moved in, and departed. The objectives of the study were "(1) the better understanding of the continuous processes of readjustment by which individuals and groups accommodate to material (technological) change; and (2) the search for trends and differentials in adjustment that have meaning for general behavioral study or for the understanding of overall social change." The seminar used two kinds of data: statistical and historical sources such as newspaper files, city directories, census returns, vital statistics, school and business records, and the like; and data gathered by a survey of 10 per cent of the households of Norristown.

Using a thirty-three-page schedule, seminar members and professional interviewers obtained information on the size, composition, and physical assets of the households, on the sociodemographic characteristics of the Norristown population, and on the attitudes and experiences of the persons interviewed. Participation in this survey and analysis of the resulting data provided an unusual opportunity for the historians in the seminar to become acquainted with the materials and techniques of the sociologists, while use of the more traditional historical data introduced the other social scientists to the problems and possibilities of historical method in their research. Goldstein reports that there was not always consensus among the representatives of the various disciplines as to the most appropriate method or interpretation, but there developed "appreciation of the differences between disciplines and the value of such differences, as well as the points at which the methods and content of the several disciplines did merge and overlap."

Over half the book is devoted to summaries of some of the numerous studies of Norristown by the seminar members, organized by Goldstein under the headings of "Population," "Impact of Change on the Economic Structure," "Impact of Change on the Individual Worker," "Acculturation and Communication," and "Other Studies." Historians interested in quantitative study of the interrelationships between technological change and the individual in modern industrial

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society should read these chapters with care. While the style is disappointingly pedestrian and sometimes opaque, many of the findings, while tentative because based on this one case study, suggest exciting hypotheses for further research in recent social, political, and economic history. It is to be hoped that the plans to return to Norristown for another intensive look will materialize.

Social Science Research Council ROWLAND L. MITCHELL, JR.

FRANKLIN D. ROOSEVELT AND THE NEW DEAL, 1932–1940. By *William E. Leuchtenburg*. [The New American Nation Series.] (New York: Harper and Row. 1963. Pp. xvii, 393. $6.00.)

IN the longest volume so far published in "The New American Nation Series," Leuchtenburg has provided an excellent concise history of the New Deal. The book is not only based upon monographic literature, but also draws substance and illustrative material from an impressive number of manuscript collections. It is comprehensive, logically organized, and written with clarity and detachment. Leuchtenburg does not define the New Deal. Implicitly he has identified it with both the foreign and domestic policies of the Roosevelt administrations from the election of 1932 through that of 1940. By including foreign policies he has provided a richer, more realistic treatment of the period than was possible in similar histories, such as those of Rauch, Brogan, Wecter, and Broadus Mitchell, which make only passing reference to foreign policy. However, whether foreign policy should be considered a part of the New Deal is questionable.

In spite of the depth of the research, Leuchtenburg has confirmed rather than changed earlier interpretations of specific aspects of Roosevelt's policies. His contribution is in synthesis, illustrative detail, and broad assessment. Until the publication of Tugwell's *The Democratic Roosevelt* in 1957, for example, it had become customary, in referring to the First and Second New Deals, to describe the Second, including the social security, Wagner, and other acts passed after 1934, as more "radical" than the First New Deal, characterized by the NRA and first AAA. Tugwell, however, insisted that during the First New Deal Roosevelt was influenced by the planners, who favored a "restructuring" of society, and that the Second New Deal was more conservative in that it reflected the views of the "atomistic" old-line Progressives led by Brandeis. Arthur M. Schlesinger, Jr., has endorsed an expanded Tugwell interpretation.

Leuchtenburg, although admitting the usefulness of the concept of the two New Deals and in general supporting Schlesinger, insists that "the extent of the shift from 1933 to 1935" has been exaggerated as has been the influence of both Tugwell and Brandeis. He asserts that the New Deal throughout was pragmatic "in its skepticism about Utopias and final solutions, its openness to experimentation, and its suspicions of the dogmas of the Establishment." However, he concludes, Roosevelt's program rested on a basic assumption "that a just society could be secured by imposing a welfare state on a capitalist foundation." Leuchtenburg

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supports fully those writers who have insisted that Roosevelt was essentially conservative in his economic views.

Leuchtenburg's appraisal of segments of the New Deal and FDR's part in them is critical: for example, Roosevelt was primarily responsible for the partial failure of the PWA; gold buying was an "ill-considered" move; social security was in many respects "astonishingly inept and conservative"; in rural areas "the New Deal was not to blame for the social system it inherited, but New Deal policies made matters worse." Indeed, few of the noteworthy accomplishments of the New Deal receive completely favorable treatment. A brilliant final chapter, however, provides convincing support for those who insist upon the importance of "the Roosevelt Reconstruction," as Leuchtenburg significantly calls it: Roosevelt, if he gave many wrong answers, "asked the right questions." He "re-created the modern presidency," made government more responsive to the will of more people, and made possible a more soundly based economy.

Leuchtenburg does not make entirely clear where he would place the New Deal historically. He quotes approvingly Carl Degler's conclusion that the New Deal "was a revolutionary response to a revolutionary situation." But he comments as follows in a footnote: "Not merely did the New Deal borrow many ideas and institutions from the Progressive Era but the New Dealers and the progressives shared more postulates and values than is commonly supposed. Nonetheless, the spirit of the 1930's seems to me to be quite different from that of the Progressive Era." He might have added that he himself has shown that even the 1920's contributed its share of ideas and institutions to the New Deal.

Duke University RICHARD L. WATSON, JR.

CHILE AND THE UNITED STATES, 1880–1962: THE EMERGENCE OF CHILE'S SOCIAL CRISIS AND THE CHALLENGE TO UNITED STATES DIPLOMACY. By *Frederick B. Pike*. [International Studies of the Committee on International Relations, University of Notre Dame.] (Notre Dame, Ind.: University of Notre Dame Press. 1963. Pp. xxviii, 466. $7.50.)

HERE is a book written with a purpose. Pike is deeply concerned about the state of United States relations with Chile. He hopes that his study will lead to a better understanding of Chile's potentially explosive social problem and of Chilean attitudes toward the United States. He believes a sharp reversal of traditional United States policies urgent lest a Castro-type revolution triumph in Chile.

The book's focus, in interpreting Chile's internal history, is upon the class struggle. Pike maintains that the growth of capitalistic enterprise and the rise of a substantial middle class have actually sharpened rather than reduced class antagonisms, that middle-class leaders (including Arturo Alessandri and Pedro Aguirre Cerda) far from destroying Chile's traditional semifeudal society have actually cooperated with the aristocracy in oppressing the nation's ever-growing *Lumpenproletariat*.

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Pike paints for the reader an ugly contemporary social landscape. At one pole are the privileged minority—the landowners, merchants, bankers, industrialists, and professional men. "This is the Chile of the rulers, and the assimilated portion of the population, a Chile noted for its stability, orderliness, sophistication, culture, and enlightened legal and political institutions." At the opposite are the internal proletariat, "in society but not of society," restless, poverty-stricken masses, inarticulate, undernourished, and demoralized. These conditions, suggests Pike, "invite a devastating social revolution."

The author treats the diplomatic history of the period 1880–1933 in the conventional way, emphasizing incidents, conferences, and treaties. Beginning with 1933, however, he departs from this approach, maintaining that these old-style diplomatic crises are no longer of overriding importance, that the increasingly critical issue in United States-Chilean relations is the social problem. He laments the United States' continued alliance with the privileged sectors of society, pointing out that democracy and capitalism in Chile have gone hand in hand in producing "outrageous social injustice." He chastises the United States' diplomatic personnel for their limited contacts with students and labor leaders and urges a prompt disengagement from the Right lest the Left gravitate to Communism. "The only policy that can save the position of the United States is to run the risk of affronting the feudalists and to pat on the back the progressively inclined—and to do it openly."

Pike is obviously going to be criticized both in Chile and the United States for being doctrinaire, for overstating his case, and for oversimplification. Whether or not one agrees with his interpretation or his sense of urgency, one must admit that this work, both in terms of historical scholarship and imagination, has enriched our understanding both of Chilean domestic history and Chile-United States relations.

The author has covered his subject well, apparently exhausting available Chilean sources; he has organized his material and has done an exceptionally fine job of writing.

University of New Mexico EDWIN LIEUWEN

A HISTORY OF ARGENTINE POLITICAL THOUGHT. By *José Luis Romero*. Introduction and translation by *Thomas F. McGann*. (Stanford, Calif.: Stanford University Press. 1963. Pp. xvii, 270. $6.50.)

FOR students of the history of political ideas, Argentina is probably the most rewarding of all the Latin American countries, not for its originality but for its wide range of borrowings from abroad, the constant readaptation of these to the local environment, and the relatively high degree of freedom and quality of the continuing political dialogue during most of Argentina's national history. The present account (first published in 1946 and translated here from the third edi-

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tion, 1959) covers the whole period from colonial times to 1955, and the author, an outstanding Latin American intellectual, is a trained historian and has had practical experience as a leading member of Argentina's Socialist party.

Of the various possible approaches to the history of ideas, the one chosen by Romero subordinates the exposition of "doctrinaire ideas" and "ideology" to an examination of political thought "as the conscience and motivating force behind attitudes and conduct. . . ." While admitting Argentina's dependence on imported ideas, he insists that these "have assumed a halo of peculiar tones—a reflection of the conditions under which we live."

This approach is valid, and it has been followed with a high degree of success in each of the three periods into which Romero divides Argentine history: "The Colonial Era," to 1810, "The Creole Era," 1810 to about 1880, and "The Alluvial Era," from about 1880 to 1955.

In most respects this is a very fine book; the only ground for cavil is that the author's frequent preoccupation with "attitudes and conduct" leads to a neglect of the ideas that he himself describes as their "motivating force." For example, closer attention to the Spanish phase of the Enlightenment might have rendered more understandable the strength of authoritarianism in early nineteenth-century Argentina. (All the best recent books on this phase are missing from the bibliography.) Again, there is no discussion of positivism or its exponents, such as Auguste Comte and Herbert Spencer, although it was an important factor in the political thought of the latter part of the nineteenth century in Argentina as in other Latin American countries. To give only one more example, lack of precision in the use of certain terms, such as fascism and nationalism, tends to obscure their distinctive character in Argentina. In this connection it might be noted that there is no reference to Manuel Gálvez, and only one reference each to Ricardo Rojas and José Ingenieros, although all three played important parts in the development of Argentine political thought, and Ingenieros wrote a four-volume history of it.

Yet Romero's book is invaluable for its broad sweep and its deft interweaving of the history of political ideas with social and economic as well as political change in Argentina. McGann has rendered an important service by making it available in an excellent English translation, with an introduction that, unlike the text, stresses comparisons with the United States, and a list of works in English supplementing the author's list of Argentine works in Spanish. The index is incomplete.

University of Pennsylvania ARTHUR P. WHITAKER

EL MOVIMIENTO EMANCIPADOR DE HISPANOAMÉRICA. Volumes I–IV, ACTAS Y PONENCIAS; Volume V, FRANCISCO DE MIRANDA Y EL ANTIGUO RÉGIMEN ESPAÑOL, by *Láutico García, S. J.* [Aca-

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demia Nacional de la Historia, Mesa redonda de la Comisión de Historia del Instituto Panamericano de Geografía e Historia, Numbers 1–5.] (Caracas: the Academia. 1961. Pp. 371; 460; 523; 502; 525.)

THE sesquicentennial of the Spanish American independence movement, celebrated by most of the major countries in 1960, inevitably brought with it a great number of special historical meetings and publications. Few historiographical breakthroughs were evident in all this activity, yet the quality and utility of the total output reflected the very real progress made in the historical sciences in Latin America during the past generation. A good example is *El movimiento emancipador de Hispanoamérica,* which contains the proceedings of a round table organized under the auspices of the Pan American Institute of Geography and History at Caracas, Venezuela, in July 1960.

Roughly half of the first volume is devoted to minutes of plenary sessions, resolutions, and opening or closing addresses—all no doubt worthwhile, although of secondary interest. The last volume is a single monograph, of which only a portion was actually presented at the round table. The rest of the five-volume set contains fifty-three papers (or in a few cases résumés of papers) that were presented by some forty-two historians. Only three Spanish American nations, Panama, Nicaragua, and the Dominican Republic, are unrepresented in the list of contributors; there are also papers from Spain, the United States, Great Britain, and Sweden. To be sure, the primary focus is on Spanish South America, with the host country, Venezuela, naturally represented by the largest number of papers (twelve). With this geographic reservation, and with the further reservation that military history per se is largely excluded, the contents offer a fairly representative cross section of recent research and writing on Spanish American independence. Sources and bibliography, political, diplomatic, economic-social, and cultural-ideological factors all are examined, and most of the leading traditional and "revisionist" schools of interpretation are represented.

Not all the papers presented at the Caracas round table, or at any other such conference, are really worth publishing. But in this case the substandard or merely mediocre items are not oppressively numerous and are usually easy to spot and skip. Certain of the papers have also appeared as journal articles or in pamphlet form, but it is still useful to bring together in one place a group of writings such as this on the theme of independence. Obviously one cannot list and evaluate all of them in a review. Even so, a few titles chosen more or less at random will perhaps suggest the scope of the collection: Edberto Oscar Acevedo, "Factores económico-regionales que produjeron la adhesión a la Revolución," an item of well-documented local history relating to Cuyo in Argentina; Lino G. Canedo, "Los franciscanos y la Independencia de Venezuela"; Arthur P. Whitaker, "Enlightenment and Spanish American Independence," an essay on the revolutionary, non- and antirevolutionary implications of the Enlightenment as applied to Spanish America; Ernesto de la Torre Villar, "Dos proyectos para la Independencia de

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Hispanoamérica: James Workman y Aaron Burr"; Pedro Grases, "La bibliografía venezolana de la Independencia y de los orígenes de la emancipación."

Volume V is a full-length book. With minor changes, it is the author's doctoral dissertation presented at the Gregorian University in Rome. It refers specifically to the years 1750–1790, which span the upbringing of the "Precursor" Francisco de Miranda in Caracas, his service in the army of Spain, and his first period of foreign wanderings. But this is not a conventional biography, or even (despite the title) a "life and times." It is an analysis, for the most part topically organized, of the stages that Miranda passed through in his progressive alienation from political and religious orthodoxy. This process was essentially complete by 1790, but García makes certain distinctions between the political and religious aspects. In the religious sphere Miranda arrived at "a deism colored with anti-Catholic and, above all, anti-Roman Christianity," largely because of his own voracious reading and other personal efforts and inclinations. In the political sphere, however, the Spanish monarchy effectively alienated Miranda by a series of slights and injustices. Not that Miranda was wholly blameless in his troubles with José de Gálvez and other Spanish officials, but García chooses to saddle them with the major responsibility.

The plan of the work naturally has its drawbacks. Thus, while García firmly rejects "the general opinion among biographers" that Miranda took part in the Spanish attack on Algiers in 1775, he offers no further discussion because military exploits lie outside his main theme. Instead, he puts the reader off with the promise of a future article. At the other extreme, possibly excessive detail is devoted to reconstructing the ecclesiastical *ambiente* of the Caracas of Miranda's youth, on the assumption, which is probably correct, but which García never fully substantiates, that Miranda absorbed therefrom a solid grounding in the Roman Catholic faith. Nevertheless, the author's method and his interpretation are on the whole quite reasonable. And he has brought to bear on his subject a very considerable amount of inedited documentation, although much of this documentation has a more direct bearing on matters of background than on Miranda personally.

A select group of documents is included in an appendix, and there are also bibliographies of both works relating to Miranda and works read by Miranda. One might wish that some of the quotations used in the volume were more fully identified, as a mere page number in the *Archivo Miranda* is not necessarily sufficient, and some passages or allusions in the text itself are bound to seem obscure, especially to the non-Mirandist. Yet in general the style is interesting and distinctive, scholarly without being dull. The book makes good reading and is a worthy addition to the literature on Miranda.

Office of Aerospace Research DAVID BUSHNELL

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HISTORIA MODERNA DE MÉXICO. EL PORFIRIATO. LA VIDA POLÍTICA EXTERIOR. Part 2. By *Daniel Cosío Villegas*. (México, D. F.: Editorial Hermes. 1963. Pp. xxxii, 967.)

Cosío Villegas' new volume is devoted to Mexico's relations with the United States, England, France, and Spain in the years from 1867 to 1911. Mexico's chief external interest in this period was to win recognition and to re-establish diplomatic relations. Naturally enough, Mexico had more dealings with its neighbor than with the European countries, and the author takes approximately one-half of his book to consider Mexican-American affairs. This volume does not touch on the relations between Mexico and the United States that grew out of their conflicting policies with regard to Central America; Cosío Villegas covered that topic at length in his preceding volume. The initial date of 1867 is somewhat misleading for although it is true that the author does touch lightly on the Juárez period, he scarcely mentions the administration of Lerdo de Tejada. Greatest stress is placed on the first administration of Porfirio Díaz (1876–1880). At this time Díaz was trying hard for American recognition, and Cosío Villegas considers the effect of the Indian raids, forced loans, and the free zone on the American response to Mexican overtures. Díaz' policy was to play for time, and he carried it through so successfully that he ultimately obtained the exchange of ministers without making any of those commitments for which the United States had been pressing. The author then proceeds quickly over the González administration and the later Díaz era, until Díaz' last years in office. He covers in great detail the meeting between Taft and Díaz at El Paso and the difficulties that the Madero revolt created for both United States and Mexican authorities. It is interesting to note that in dealing with the latter subject he has to rely most heavily on the American State Department archives.

The remaining half of the book concerns Mexico's relations with the three countries of Europe, where the principal objective was to win recognition and put diplomatic relations on a normal footing. After Spain agreed to restore broken relations, the only problem that caused real trouble was Mexican neutrality during Spain's quarrels with Cuba. While it took longer to work out a solution with France, once the matter was resolved the Mexicans forgot the years of intervention and returned to their old custom of adoring all things French. In dealing with England, Cosío Villegas devotes three hundred pages chiefly to the decisive years between 1882 and 1884 when negotiations brought about the resumption of diplomatic intercourse. The successful outcome of Anglo-Mexican discussions was facilitated by strong motivation on both sides: England was anxious to increase its commercial and financial ties with Mexico; the Mexicans welcomed an opportunity to lessen their dependence on the United States.

As Cosío Villegas points out, there are few articles and no books on Mexican relations with European countries, and he has certainly made a major contribution in this area. In Mexican-American relations he has added much detail to in-

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formation previously available. He used the archives in the United States, Europe, and Mexico and also personal papers in his extensive research. The only possible complaint about a book of such length is that it is too detailed.

University of Missouri WALTER V. SCHOLES

ENTREPRENEURSHIP IN ARGENTINE CULTURE: TORCUATO DI TELLA AND S.I.A.M. By *Thomas C. Cochran* and *Ruben E. Reina. Sue Nuttall,* Research Assistant. (Philadelphia: University of Pennsylvania Press. 1962. Pp. xi, 338. $7.50.)

DI TELLA was an Italian immigrant to Argentina who began his dazzling entrepreneurial career in 1911 at the age of eighteen by organizing a company to manufacture dough-kneading machines. When he died, in 1948, his companies, foreign and domestic, organized under SIAM (*Sociedad Industrial Americana de Maquinarias*), had annual sales of fifteen million dollars, and he was the largest general machine manufacturer in South America. Since 1948 the company has expanded greatly and has spawned such progeny, rare in Latin America, as a philanthropic foundation and a research institute.

The account of these events by Cochran and Reina is a substantial success. To study entrepreneurship in a Latin American cultural context is commendable. No similar company-owner study exists. The authors have ventured into the Latin American no man's land of restricted family and business files and personal recollection. They deserve the gratitude of those interested in economic history and development, as does the Di Tella family, whose liberality made possible this scholarly break-through.

The documentation is ample, and the narrative is organized in a chronological pattern: the Argentine historical background; the early years of the immigrant; the critical, expansive years, 1928–1938; the final decade of Di Tella's life; the decade following his death; and considerations of the relationships of Argentine entrepreneurship, industrialization, and national culture.

Through the study runs a clear motif: Argentine business culture, as represented by SIAM, is impregnated with the *personalismo* characteristic of other aspects of Latin American society—personalism of a benevolent, paternalistic sort, in intent, marked by such evidences as the early introduction by Di Tella of "fringe benefits" for his employees. Then the countertheme appears: under the dual influences of Perón's labor policies and the United States' industrialization model, the far-flung SIAM organization has moved toward impersonal production planning and impersonal labor relations.

Numerous suggestive issues are introduced: the "extended family" as a necessary substitute for a shortage of outside managerial talent; Di Tella's entrepreneurial daring, mixed with implications of his good connections within the Argentine government; and emphasis on his role as the *patrón*—the *simpático* owner-father (yet this image is curiously contradicted by some references in the text and

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in the appendixes). In a culture noted for its imitative and adaptive capacities, it is important to find that the most important business enterprise was also imitative and adaptive. Most of Di Tella's production was based on the modification of foreign-licensed products such as refrigerators and gasoline pumps, which he introduced with bold imagination into the underdeveloped Argentine market of thirty or forty years ago. Some substantial part of Argentina's twentieth-century economic revolution is due to Di Tella.

Entrepreneurship in Argentine Culture is a landmark in the coming of age of Latin American studies. It is regrettable, therefore, that the book is marred by broken type, faulty inking, lack of Spanish accent marks, and extraordinarily erratic page composition.

University of Texas THOMAS F. MCGANN

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* * * Other Recent Publications * * *

BOOKS

General

INTERNATIONAL BIBLIOGRAPHY OF HISTORICAL SCIENCES. Volume XXVIII, 1959, including some publications of previous years. Edited with the contribution of the national committees by *Michel François* and *Nicolas Tolu* for the International Committee of Historical Sciences, Lausanne. [Published with the assistance of UNESCO, and under the patronage of the International Council for Philosophy and Humanistic Studies.] (Paris: Armand Colin. 1961. Pp. xxvii, 447.) There are no surprises in this twenty-eighth volume of a familiar, and once a pioneering, bibliographical series. From the standpoint of method and presentation, the *International Bibliography* has stood still since its first volume, which covered the writings of the year 1926. This is unfortunate because of the remarkable growth in recent years in international bibliographies, several of them, as this particular volume in the series, supported by UNESCO. UNESCO's *International Social Science Bibliographies* (published in such fields as political science, sociology, social and cultural anthropology, and economics) have, it seems to me, made it imperative for the editors of the *International Bibliography of Historical Sciences* to reappraise the place of their work in the total current bibliographical picture. For example, why continue to include a section like the one entitled "America," with two pages of anthropological writings, better covered in the bibliography on social and cultural anthropology. The UNESCO bibliographies have much to offer from the standpoint of presentation. They are by and large better arranged, better indexed, easier on the eyes; they include occasional notes that help to elucidate hard to fathom titles. So far only timid steps in the direction of change have been taken by the editors of the present work. For example, country subdivisions have been inserted in cumbersome sections on modern economic and social history and modern intellectual history. But short of a total overhaul, it is difficult to foresee much to be gained by continuing this bibliography in its present form. The expense, not to mention painstaking care, of preparing an edition of nine hundred copies of this work is difficult to justify when one considers that many copies end up on the shelves of libraries which have good card catalogues with subject headings in both broader and narrower fields and cards that reach back and forward further in time.

Washington, D. C. NATHAN A. HAVERSTOCK

"GESCHICHTLICHKEIT": WEGE UND IRRWEGE EINES BEGRIFFS. By *Gerhard Bauer*. [Die kleinen de-Gruyter-Bände, Number 3.] (Berlin: Walter de Gruyter & Co. 1963. Pp. vii, 207.) *Geschichtlichkeit* can hardly be translated into English. As Bauer points out, the concept is typically German and can be expressed only with great difficulty in any other language. The reason for this difficulty becomes evident in Bauer's interesting and comprehensive discussion of the history of *Geschichtlichkeit*. Only the Germans have a word for it because, until recently, only they have taken seriously the notion that reality, at least that reality which is human existence, is ultimately historical. The history of the concept really begins with two German thinkers

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of the late nineteenth century, Wilhelm Dilthey and Graf Paul Yorck von Wartenburg, though elements of it are found in nineteenth-century German historians, German romantics, and German idealists (especially Hegel), and even in Vico, almost the only non-German discussed at any length. For Dilthey and Yorck *Geschichtlichkeit* has scientific significance. It refers to what distinguishes the subject matter of the *Geisteswissenschaften* from that of the *Naturwissenschaften*. Through subsequent elaboration in the thought of Husserl, Scheler, Simmel, Spengler, Heidegger, Jaspers, and a host of others, the concept gradually changed from a scientific notion to a personal one. Indeed it might be said to have been transformed into the radical subjectivity of prewar existentialism, where it is manifested as "situation," "decision," "encounter," and the like. Since World War II there has been a tendency to revert to a more scientific, objective, or neutral use of the concept in the *Geisteswissenschaften*, though this development is too recent to permit Bauer's offering more than mere suggestions about it.

University of Oregon LLOYD R. SORENSON

A SHORT HISTORY OF SOCIOLOGY. By *Heinz Maus*. (New York: Philosophical Library. 1962. Pp. viii, 226. $6.00.) Even if the reader had hoped for an analytical history of sociological ideas, he need not be entirely disappointed by this book. While the author has done little more than to present the filiation of sociological ideas in simple terms and to make a catalogue of the sociologists who are associated with these ideas, such information does help a novice in the field to locate people and capsule versions of their ideas. The capsules, to be sure, seemed to me to be a little misleading, for Maus does not hesitate to lace his brief accounts of ideas with implicit or explicit judgments: thus, Veblen's work is dealt with in *one* paragraph and is described as "in danger of becoming fashionable," while Charles A. Ellwood gets *four* paragraphs, because, presumably, no danger exists that anyone might pay too much attention to him. Be that as it may, if one wishes to know who Durkheim's principal students were, or his chief critics, such information is easily found, as are the names of recent important Danish sociologists, or contemporary Germans working in the field of the sociology of the family. As a sociological *Who's Who* the book is more up to date for Europe than for the United States: "young" French sociologists, for example, are mentioned, but Americans of comparable age and distinction are not. Maus's book is organized according to the development of sociological theory in the different nations, but if there are national differences, they are not well analyzed; nor is the development of sociology related to the intellectual traditions or to the political and educational institutions of the various countries. Patterns of cross-national interdependence are hardly explored, although cases of cross-national interaction abound. Though in the introduction the author appears to promise something in the way of a sociological history of sociology, he delivers much less than that. Finally, it must be said that in a quasi-reference book such as this the poor quality of the index is especially to be regretted.

International Encyclopedia of the Social Sciences ELINOR G. BARBER

ESSAYS IN MEMORY OF SIR HILARY JENKINSON. Edited by *Albert E. J. Hollaender*. ([London: Society of Archivists.] 1962. Pp. 189.) Originally conceived as an eightieth-birthday tribute to Sir Hilary Jenkinson, this collection of a dozen essays became, after Jenkinson's death on March 5, 1961, a memorial to his work and contribution as an archivist and scholar. Testimony to the respect and affection held for Jenkinson by his colleagues, the essays are impressive evidence of the wide range and decisive nature of his influence on archival affairs in Great Britain and the English-speaking world. No account is given of Jenkinson's half century of association with the British Public Record Office, from which he retired as deputy keeper in 1954, but his efforts—on the whole, highly successful—in behalf of local records in Great Britain, his

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promotion of proper facilities and sound archival principles for their care, his insistence on professional training for archivists, his contributions to the study of sigillography and paleography, and his work as War Office adviser on archives are covered in some detail. American archivists will be most interested in two essays by Commonwealth archivists: one by W. Kaye Lamb, Dominion Archivist of Canada, on the fine art of the destruction of records, in which exception is taken to Jenkinson's precept that the archivist should not become involved with this troublesome question; the other by Ian Maclean, Chief Archivist, Commonwealth of Australia, a perceptive analysis of Jenkinson's *Manual of Archive Administration* based on Australian experience. Of considerable value to many who have wondered about the arrangements made for the records of the nationalized industries in Great Britain is the essay on the archives of the British Transport Commission. Archivists everywhere will enjoy the initial essay by Roger Ellis, a delightful account of the vicissitudes encountered in the design and construction of the British Public Record Office in London.

National Archives ROBERT H. BAHMER

BORDERLANDS. By *Delia* and *Ferdinand Kuhn*. (New York: Alfred A. Knopf. 1962. Pp. xxi, 335, xv. $6.75.) The borderlands are six restless regions lying between the Arctic and the tropics in the long shadow of China or the Soviet Union: Hokkaido, the Sulu Islands, northern Burma, Sikkim, Afghan Turkestan, and eastern Turkey. All these places are underdeveloped; none promises to be the El Dorado of tomorrow. They are not ready for an industrial revolution; they need only such elemental things as roads, schools, and clinics. In their isolation, they have scarcely been touched by the revolution of rising expectations. The frontier—any frontier—implies "feudin' and fightin' " where a man's safety depends upon the accuracy of his gun or the speed of his sail. Any kind of good government, let alone a democracy, is rare. But the quarrels of the borderlands are explosive because they involve the opposite sides of the bipolar world's Great Power systems. Some borderlands are now quasi colonies in countries that were themselves colonies until recently. When, for example, the Shans speak of a "foreign" language, a "foreign" race, or a "foreign" army, they do not mean the British; they mean the Burmans. Sikkim is under India, as India was once under England. The Pathans want independence from Pakistan, as Pakistan once wanted independence from its own foreign masters. When a frontiersman speaks of "exploitation" he means exploitation by his own government. He is inclined to be resentful when his native state pays more in taxes than it receives in benefits from the national capital. The Kuhns are at their best in describing the complex ethnic structure of the borderlands. How foolish to speak of "Chinese" on one side of the border and "Indians" on the other! "Chinese" may mean dozens of tribesmen, or "Indians" may mean hundreds of distinct peoples who are as different from each other as "hawks and pigeons." Teachers will enjoy the book because of its wealth of anecdote and color; students will like it because of its interesting style. The authors offer provocative observations on the role of the borderlands in the cold war. Both the Communists and the anti-Communists want the support of the people on the frontier. Americans work "from the inside out" while Russians or Chinese approach their neighbors "from the outside in." It is a contest between aid to central governments (which may never reach the borderlands) versus Communist tactics of subversion and infiltration. Progress in this kind of cold war is more likely to result from assistance in economic development than the subtleties of ideological conflict.

Stanford University CLAUDE A. BUSS

DIE GELBE GEFAHR: GESCHICHTE EINES SCHLAGWORTS; STUDIEN ZUM IMPERIALISTISCHEN DENKEN. By *Heinz Gollwitzer*. (Göttingen: Vanden-

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hoeck & Ruprecht. 1962. Pp. 268. DM 19.80.) Professor Gollwitzer of the University of Münster has written a suggestive but, on the whole, rather unsatisfying book on the history of the slogan "the yellow peril." His book covers the period from the 1870's to the First World War. He devotes a chapter each to American, British, French, German, and Russian writers on the subject, and much of his material, drawn from a wide variety of sources, is fresh and interesting. (The self-proclaimed author of the slogan, we learn, was William II.) The book, however, also has a serious defect. Like so much *Geistesgeschichte,* it never properly relates the ideas and opinions discussed to the politics and diplomacy of the period. The result is that we never get a clear picture of the contemporary importance of these ideas and opinions. After all, we want to know, for instance, not only what Mahan and Tirpitz, Theodore Roosevelt and William II thought and wrote about "the yellow peril," which Gollwitzer tells us; we also want to know to what extent their domestic and foreign policies were shaped or influenced by such concern, which he fails to tell us. Fear of "the yellow peril," Gollwitzer maintains, played a greater role in American politics, foreign and domestic, than in that of any other Great Power. Perhaps so, but there is no proof of that in his book.

Rice University FRANCIS L. LOEWENHEIM

FRIEDRICH ENGELS UND DIE INTERNATIONALE ARBEITERBEWEGUNG. Edited by *Karl Obermann* and *Ursula Herrmann.* [Deutsche Akademie der Wissenschaften zu Berlin. Schriften der deutschen Sektion der Kommission der Historiker der DDR und der UdSSR, Number 1.] (Berlin: Akademie-Verlag. 1962. Pp. 156. DM 7.50.) This commemorative symposium, sponsored by the German section of the commission of East German and Soviet historians, embraces five essays dealing with various aspects of Engels' career. The first of these, by E. A. Stepanowa, describes Engels' role as a teacher, an exponent of Marxism, and a leader of the working class. Obermann's essay on Engels' interest in German political unification and democracy focuses on the Revolution of 1848. J. I. Linkow discusses Engels' influence on the agrarian revolutionary movement in Russia. Horst Bartel emphasizes Engels' crusade against revisionism as the forerunner of the Liebknechts, of Bebel, Luxemburg, Thälmann, and Pieck. G. N. Sewostjanow deals with Engels' contacts with American socialists and analyzes the hazards now confronting Communism in the United States. Based on well-chosen extracts from Engels' correspondence, from the Merseberg archives, from the daily and periodical press, and commentaries of various Left-wing spokesmen, the essays emphasize the broad sweep of Engels' interests in revolutionary movements. Unfortunately, the authors do not present much information on the genesis of those interests and that dedication to social revolution of a man who, much like his collaborator Marx, was reared in the bourgeois and pietistic traditions of early nineteenth-century Westphalian society.

Pennsylvania State University ALFRED G. PUNDT

GERMANY AND THE CENTRAL POWERS IN THE WORLD WAR 1914–1918. By *Walther Hubatsch.* Edited by *Oswald P. Backus.* Introduction by *Henry Cord Meyer.* (Lawrence: University of Kansas Publications, Social Science Studies. 1963. Pp. xv, 133. $3.00.) This book consists of lectures given in English at the University of Kansas by a member of the department of history of the University of Bonn, who in 1955 published *Der Weltkrieg, 1914–18.* It is an uneven performance. The sections on military operations, which comprise at least half of the book, are clear and objective; if the supreme command comes in for a good deal of criticism, the author rejects the thesis of General Hoffmann that the struggle was a "war of lost opportunities" and says that it was "rather a world war which the Central Powers simply could not win

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in view of the overwhelming power of the Entente." He also makes much of the conflicts between the German and the Austro-Hungarian general staffs. On the political side, the book is less satisfactory. The two sections on the origins of the war reflect German attitudes of the 1920's. The views of Fritz Fischer, whose *Griff nach der Weltmacht* (1961) is highly critical of German diplomacy before 1914, are rejected out of hand. Luigi Albertini's *The Origins of the War of 1914* (1953) is mentioned in the bibliographical notes, but no account is taken of his arguments. Ludwig Dehio's *Germany and World Politics in the Twentieth Century* (1959), which recognized a large measure of German responsibility for the war, is not even mentioned. The diplomatic history of the war years is inadequately sketched. Most shocking of all is the statement that "no chancellor adopted a program of annexation during the war," although Fischer has published numerous documents from the German archives that leave no doubt that from September 1914 to July 1918 both the civil government and the general staff aimed at large-scale annexations. The further statement that "on March 27, 1917 Bethmann Hollweg [the German Chancellor] and Czernin [the Austro-Hungarian Foreign Minister] had agreed in Vienna to aim at a *status quo ante bellum* and to consider annexations in the east and in Romania only if things went very well" is misleading. Fischer shows that it was Czernin, not Bethmann, who was for the *status quo ante bellum,* and that Germany retained a free hand in the west. The reality of the situation was revealed at Kreuznach a month later when the Central Powers agreed upon an extensive program of expansion and annexation both east and west (an agreement revealed many years ago). The effort to justify the Treaty of Brest-Litovsk as a vindication of the right of self-determination is specious. If Hubatsch can write that "Wilson neither understood the circumstances in which Germany and Austria-Hungary found themselves nor was interested therein," there is nothing in his book to show that he has any understanding of Wilson or the policy of the United States.

University of Chicago BERNADOTTE E. SCHMITT

BRITAIN AND THE U.S.A. By *Herbert Nicholas.* [The Albert Shaw Lectures on Diplomatic History, 1961.] (Baltimore: Johns Hopkins Press. 1963. Pp. 191. $5.00.) Originally lectures by a fellow of St. Antony's College, Oxford, the chapters in this book intelligently discuss the transformation of Anglo-American relations from the Munich crisis of 1938 to 1961. They provide a useful, if rather dull, synthesis with the normal insights one would expect from someone who has specialized in the subject. As a topical survey of the various important points of contact from the atomic bomb and military alliances to economic problems, they have their usefulness. It may be noted that the author's sources are heavily American, a fact that emphasizes the difference in attitude between the two governments in regard to the publication of official documents. Some of them are official papers, but many are memoirs. Basically, the difficulty with this sort of book is that one expects the products of a distinguished lecture series to be exceptional, to have wit and valuable, sparkling new insights. But faculty members and reviewers who are themselves working in the field demand too much and rarely find synthesizing lectures in their own field very stimulating. On the other hand, such presentations in a neighboring area or in undergraduate and even graduate students' eyes can be valuable.

Kansas State University ROBIN HIGHAM

Ancient and Medieval

GIBEON, WHERE THE SUN STOOD STILL: THE DISCOVERY OF THE BIBLICAL CITY. By *James B. Pritchard.* (Princeton, N. J.: Princeton University Press.

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1962. Pp. xix, 176. $5.75.) Palestinian digging is no longer primarily concerned with proving or disproving the historicity of the Old Testament. It has become abundantly clear that where the Bible chronicles events, these events are real, the place names and most of the personal names are genuine, and only tribal bias and storyteller's romance cloud the strict veracity of the narrative. Today the events with all their human characters have receded into the irrevocable past, but their geologic and geographic settings still remain, and amid these there survives underground a fantastically haphazard assortment of material odds and ends, in which the past is still the present—even as the ancient place names, however transformed, continue to live in the mouths of today's inhabitants. It is from the evidence of such fragmentary excerpts out of an incalculably more abundant past that archaeology has set itself the task of constructing a scaffolding of continuous cultural history for Palestine, to which the Old Testament contributes, but which it does not dictate. Where Biblical testimony is lacking, archaeology undertakes to fill the gap; where this testimony is vague or purely casual, archaeology adds the needful supplement. Thus, the Bible makes no mention of ancient Gibeon for the period between Solomon's reign and the Babylonian Exile—nearly four centuries of silence, which recent digging proves to have been the city's most prosperous era. The historian who wants to examine by what shifts and devices his archaeological colleagues cope with the seemingly senseless fragmentation of a thrice millennial past will find Pritchard's unpretentious but exceptionally well-presented and completely authoritative account of four recent campaigns of digging a sparsely settled, saddle-shaped hill a few miles outside of Jerusalem a brilliant lamp of intelligent enlightenment. I know no better or more readable survey of modern excavational procedure in all its unexcitingly exacting routine and intense concentration on every chit of germinable evidence. At the same time, the precision of detail is presented with such simple clarity, and the events and vicissitudes attendant on excavation are so engagingly recounted, that *Gibeon* may be read with enjoyment for its own sake as a picture of a still-primitive Arab community and a pleasant narrative of Western intrusion on Eastern ways of life.

Bryn Mawr College RHYS CARPENTER

THE WORLD OF HERODOTUS. By *Aubrey de Sélincourt*. (Boston: Little, Brown and Company. 1962. Pp. 392. $6.50.) Because of his universal outlook, boundless curiosity, and general lack of bias, Herodotus was probably the only Greek historian whom most moderns would really enjoy meeting. In an urbane and witty book, Sélincourt insists that the Greeks lacked a sense of history and that Herodotus was the greatest and indeed the only Hellenic historian. Thucydides is dismissed as "tendentious in matter, parochial in scope, and difficult to read"; "No sensitive modern reader . . . could fail to realize that for sheer intellectual stature Herodotus was by far his superior." After this introductory broadside, Sélincourt examines the subject matter of Herodotus with zest and learning, adding lively chapters on the ways of Hellas and concluding with a moralistic denunciation of imperial Athens for "betraying" the Greek ideal. As an "amateur," Sélincourt avoids pedantry and often employs the deceptive Herodotean comment, "I am told." Despite occasional factual errors, the book is strongest in the chapters on Greece and weakest on the Near East, where the author fails to employ the Behistun inscription and the Cyrus cylinder, though the Themistocles decree appears in a discussion of Salamis. By omitting or simplifying historiographic problems, Sélincourt suggests that Herodotus was more accurate than was always the case. Many scholars will dispute Sélincourt's glorification of the seventh and sixth centuries B.C. as a "sunlit" era in which the status of women was high and pederasty rare. Apparently long familiarity with Herodotus has made him receptive to moralism, for Sélincourt believes

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that the Greeks were unfitted for empire or federalism and that Athens was guilty of *hubris* and duly punished. While general readers may accept too many of his generalities as facts, scholars and students will enjoy Sélincourt's pithy remarks and disdain for cliché. His general thesis is sound and not always appreciated: "Herodotus' book, with its countless digressions and asides, is as revealing as to look for the first time at a drop of water under a microscope."

University of Southern California THOMAS W. AFRICA

RHETORIC AT ROME: A HISTORICAL SURVEY. By *M. L. Clarke*. (New York: Barnes and Noble. 1963. Pp. vii, 203. $4.00.) The author of this monograph is professor of Latin in the University College of North Wales at Bangor. Deploring the neglect into which the Roman rhetoricians have fallen of late, he attempts to show their importance in the intellectual history of Rome. Though the formal study of rhetoric began at Rome only in the second century B.C., when Greek rhetoricians began teaching in the city, Roman orators had been making speeches long before this time and had developed their own style of oratory. The ancient rules were well expressed by Cato the Censor, who declared simply that a good orator was a *vir bonus discendi peritus* and advised the orator *rem tene, verba sequentur*. In the days of the Republic, Roman orators studied Greek rhetoric, to be sure, but when delivering speeches they usually followed the ancient Roman practices. Cicero and Quintilian did much to domesticate Greek ideas, however, and in the second century A.D. the rhetoricians took over. Scholars have often pictured this victory of Greek rhetoric as an unmitigated disaster for Rome and for the world, but Clarke is not sure. He believes that Rome could have survived "even the follies of the lesser rhetoricians," and he concludes his study by remarking that "rhetoric is well enough if kept within limits."

University of Illinois J. W. SWAIN

NICOLAUS OF DAMASCUS. By *Ben Zion Wacholder*. [University of California Publications in History, Volume LXXV.] (Berkeley and Los Angeles: University of California Press. 1962. Pp. viii, 150. $3.00.) This monograph brings together virtually all that can be known about the life and work of Nicolaus of Damascus. Nicolaus was one of those polymaths who attempted to synthesize the culture of the past, not uncommon during Hellenistic and Roman times. He lived during the first century B. C. and was in the service of Herod. His intellectual activities lay in his studies of Aristotle and the whole Peripatetic school as well as in history. He wrote what may be described perhaps as the most voluminous history in Greek. To what extent his work was characterized by profundity is difficult to say, for virtually all of it has perished. Jaeger thinks that Nicolaus did not understand Aristotle, but this is a matter that cannot be proven. Nicolaus was the author of a eulogistic biography of Augustus, large fragments of which have survived. These fragments contribute very little to our understanding of that Emperor, but the biography as a literary form was rather influential. Constantine Porphyrogenitus, the scholarly Byzantine Emperor of the tenth century, used it as a model when he wrote a biography of his grandfather, Basil I. Wacholder was no doubt attracted to the study of Nicolaus because the latter was one of the principal sources of Josephus in his exposition of Jewish history.

Rutgers University PETER CHARANIS

THE SILVER-PLATED AGE. By *Tom B. Jones*. (Sandoval, N. M.: Coronado Press. 1962. Pp. 228. $1.95.) The author of this volume obviously had a fine time writing it. His book is primarily a history of the intellectual class in the ancient world during the Pax Romana of the second century, but he finds many occasions to compare conditions

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then with those prevailing today under what he calls the "Eirene Americane." Many features of the Roman Peace may be praised, but its intellectual life was sterile. In the mere production of books it was the most prolific century of antiquity, but the writers of these books had little or nothing to say. Jones attributes this sterility to the faulty education then in vogue, under which pupils were forced to study the classics of previous centuries, but were not encouraged to express opinions of their own. After giving a critical description of Greek and Latin schools, and lists of the books actually studied in them, Jones discusses the products of these schools, devoting separate chapters to the rhetoricians, the philosophers, the lawyers (whom he calls "vultures"), and the doctors (in a chapter entitled "Pox Romana"). He declares that the writing of history was then considered a polite avocation requiring neither talent nor training and that the study of natural science culminated in astrology. The concluding chapter (enigmatically entitled "The Labyrinth") deals with early Christianity and especially with Gnosticism. The book shows wide reading in the ancient authors, though there are a few surprising gaps. The mystery religions are not mentioned, and Cumont's name does not appear in the bibliography.

University of Illinois J. W. SWAIN

SOLDIER AND CIVILIAN IN THE LATER ROMAN EMPIRE. By *Ramsay MacMullen*. [Harvard Historical Monographs, Number 52.] (Cambridge, Mass.: Harvard University Press. 1963. Pp. vii, 217. $5.00.) This is a study of nonmilitary activities of the army and of militarization of the state and society roughly from A.D. 200 to 400. The first three chapters are devoted to soldiers as farmers, artisans, technicians, builders, clerks, and administrators. The next three deal with the cultural, social, and, especially, economic consequences of the growing concentration of troops in towns; the role of soldiers and veterans in the political life of cities; and the private armies and fortified farms that became necessary in the countryside. The last chapter is partly a summary, partly a series of further illustrations of the militarization of the Empire. Despite its modest size, the monograph is the most comprehensive study of its subject, one closely connected with other profound changes that took place in the period covered. It is based on wide reading and a careful collection of diverse and scattered evidence, recorded and discussed in many long notes. The author is independent in his judgment. His presentation is lively, clear, and concrete. It is impossible to discuss points of detail here, but some larger questions may be noted briefly. In general, it seems doubtful whether soldiers gave much time to farming in the third century; more specifically, the *Historia Augusta* is not adequate evidence for concluding that *limitanei* were introduced by Severus Alexander. In making distinctions between military and civilian functions, MacMullen seems much more severe than Romans in the early Empire. Not surprisingly, all examples of militarization are not equally convincing. For example, the *acclamatio* is found in a purely civilian setting long before Constantine. But the point to emphasize in concluding is that the book is a substantial contribution to our understanding of important developments.

Columbia University J. F. GILLIAM

RABAD OF POSQUIÈRES: A TWELFTH-CENTURY TALMUDIST. By *Isadore Twersky*. [Harvard Semitic Series, Volume XVIII.] (Cambridge, Mass.: Harvard University Press. 1962. Pp. xii, 336. $7.50.) The twelfth century witnessed a burst of striking creativity in the flourishing Jewish communities of the Provence. Stirred by the vibrant growth of such cities as Marseilles, Montpellier, Béziers, Lunel, Narbonne, and Carcassonne, the Jewish communities within these cities pulsated with intense energy and won recognition throughout the world-wide Jewish Diaspora as citadels of Jewish

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legal learning. And among the scholars responsible for this recognition, no one could lay better claim to pre-eminence than Rabbi Abraham ben David of Posquières (about 1120–1198). Indeed, his intellectual achievement refracts all those features which made for a distinctive and distinguished Provence Judaism. Twersky has attempted in his *Rabad* (an acronym for Rabbi Abraham ben David) *of Posquières* to convey both the genius of this scholar and of the Provençal society that made his individualistic expression possible. He has collected all the data extant on the life of Rabad; he has made a penetrating analysis of all his extant writings; he has illuminated Rabad's celebrated critique of Maimonides' doctrinal and legal code, the *Mishneh Torah;* he has delved into the sources of Rabad's opinions; he has sought to establish Rabad's position with respect to philosophy and cabala (mystical doctrine). Rabad emerges from Twersky's work as a casuistical, dialectical legal mind of the first order, a brilliant exemplar of a method by which authoritative legal texts yield novel insights and original legislation. Unfortunately Twersky's erudition will be appreciated only by the handful of scholars who are already aware of Rabad's genius. For others, it will be largely beyond comprehension. And this is not the author's fault. Rabad's especial competence was his mastery of Talmudic and Rabbinic sources. Indeed, his major quarrel with Maimonides was that the latter had not only failed to quote his sources, but had refrained from demonstrating his mode of legal reasoning. Rabad's "Notes" consequently presuppose a thorough awareness on the part of his reader of the vast legal literature. Similarly, every one of his other works was written for legal experts. To fully appreciate Rabad, then, one must know the legal problems that his dialectical sharpness clarified. Since Twersky could scarcely be expected to provide a multivolumed summation of the Jewish law to which Rabad addresses himself, one must be grateful for this volume which calls the attention of historians to a long-neglected legal genius and to the as yet unexplored golden age of Provençal Jewry.

Hebrew Union College-Jewish Institute of Religion ELLIS RIVKIN

DIE PÄPSTLICHEN LEGATEN IN FRANKREICH VOM SCHISMA ANAKLETS II. BIS ZUM TODE COELESTINS III. (1130–1198). By *Wilhelm Janssen.* [Kölner historische Abhandlungen, Number 6.] (Köln Graz: Böhlau Verlag. 1961. Pp. vii, 206. DM 18.) This excellent volume carries on Schieffer's work (*Die päpstlichen Legaten in Frankreich vom Vertrage von Meersen (870) bis zum Schisma von 1130* [1935]) to the accession of Innocent III. The book makes a valuable contribution to the historical discussion of a most important group of men in the upper echelons of the fastest developing institution of twelfth-century Europe. The growing significance of the college of cardinals was reflected increasingly in the variegated activities of the papal legates. Such activities shifted gradually from the "ecclesiastical-reforming" missions of the post-Gregorian epoch to the preponderantly "diplomatic-political." Alexander III marked the dividing line. All available source materials from a vast array are used effectively. The book is organized chronologically by pontificates; it divides legates of primary from secondary rank. A third division sums up the considered views of the author on the nature of legatine authority, selection of legates, and their activities. Ample bibliographical notes and an index complete a book that should soon be found on the shelves of any library with any pretensions to serious interest in the Middle Ages.

University of Pittsburgh GEORGE B. FOWLER

UNE VILLE DEVANT LA PESTE: ORVIETO ET LA PESTE NOIRE DE 1348. By *Élisabeth Carpentier.* [École Pratique des Hautes Études, VI[e] Section. Centre de recherches historiques. Démographies et sociétés, Volume VII.] (Paris: S.E.V.P.E.N. 1962. Pp. 284.) Orvieto, a solitary town perched high on the volcanic rock of central

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Italy has, after centuries of neglect, been the subject of two recent historical monographs. Except for the energetic nineteenth-century historian Fumi, editor of the chronicles of the city (*Ephemerides Urbevetanae*) and the *Codice Diplomatico,* no other scholar has been notably impressed with anything other than the gaudy cathedral or the heady white wine. Perhaps the fact that Daniel Waley and the author of the present monograph did indeed betake themselves to the *Archivio Comunale* may be something of a commentary on our own times. Waley's study traces the political history of the city from 1157, when the pope accorded recognition to the commune, until 1334, when the despotism of the Monaldeschi superseded the self-governing regime. Now, the narrative so unobtrusively and effectively presented by Waley is continued. Based almost exclusively upon documents from the *Riformagioni* (minutes of the communal council meetings), Carpentier's work relates the day-to-day activities of the signory as it attempted to treat the various problems confronting Orvieto during that worst of all possible times, the late 1330's through the early 1350's. Business depression, shortages of public funds, a scarcity of labor, increasing costs, and a declining population were but a few of the items in a melancholy catalogue of tragedies. Couple these with famine in 1340, flood in 1346, famine in 1347, and for those who survived all the disasters flesh can be heir to, the Black Death was, of course, to come. The principal success of this book lies in its account of the actions of the legislative councils during those nerve-racking years. It must be said, judging from the vigorous and intelligent responses of political figures to the ravages of the Black Death, that the Orvietani recovered and went on resolutely with the business of living. The document of May 1350 is worthy of scrutiny and reveals that the signory was well aware of the most pressing economic problems. Moreover, liberal efforts to fix wages and prices, and even to peg the value of the lira, bespoke much for the wisdom of the rulers of the city. The author's excursions into the psychology of crisis and her attempts to correlate resurgencies of class hatreds with the onslaught of epidemic follow the course charted by Baehrel in his two recent articles, "Epidémie et terreur," and "La haine de classe en temps d'épidémie." Further, one encounters a fascination for the beguiling approach of Lucien Levbre, especially as set forth in his "La sensibilité et l'histoire." Unfortunately, evidence for Orvieto is starkly public, and in the absence of notarial records and private papers, one cannot hope to capture the intimacy of feeling and reaction essential for a depiction of this "crise psychologique."

Western Reserve University MARVIN B. BECKER

LE SENTIMENT RELIGIEUX EN FLANDRE À LA FIN DU MOYEN-ÂGE. By *Jacques Toussaert.* Preface by *Michel Mollat.* [Civilisations d'hier et d'aujourd'hui.] (Paris: Librairie Plon. 1963. Pp. 886. 30.85 fr.) Whoever has the patience and time to read a book thick with evidence, often less than objective in discussing matters of faith, and sometimes punctuated with irrelevant sociological and philosophical observations will be partially rewarded. The aim of this book is to ascertain the depth and genuineness of religious sentiment in maritime Flanders from the early fourteenth century to the breakup of religious unity in the early sixteenth century. In his amassing of evidence and his deductions Toussaert increases our knowledge of the crucial centuries that saw the transition from the medieval to the early modern world. Toussaert tests the traditional view, a view expressed even recently in the massive work of E. de Moreau on the history of the Church in Belgium, that medieval Flanders, more than the rest of Western Europe, was a bastion of deep and vibrant Christian faith. Studying all the evidence bearing upon religious instruction of the clergy and laity, the practice of the sacraments, collective devotions, individual faith, regulatory ordinances, artistic expression, and the religious reactions of the people during periods of

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physical, social, economic, and political crises, Toussaert concludes that medieval Flanders gravely lacked a fundamental religious formation in the realm of sensations, ideas, conscience, and faith. Consumed by poverty and the struggle to exist, the people were incapable of the religious sentiment generally ascribed to them. Instead of a profound and constant faith there was emotionalism, spasmodic eruption of fervor bordering on paganism, superstition, and excessive instability. Up to this point Toussaert's argument is sound. When, however, he attempts to establish a rapport between this evidence and social, economic, and political phenomena, he becomes confused and entangled in historical conflicts spawned by the nineteenth- and twentieth-century linguistic and political problems of Belgium. To cite two examples of what is meant, numerous Belgian historians would not accept the statement that 1302 may be considered as the date "de naissance du flamingantisme" or that the peasants understood their fight against Philip the Fair and the Leliaerts as a fight for the preservation of the democratic institutions dear to the Flemish people. This book certainly dispels the illusion that, amidst a Europe crumbling in its traditional faith, Flanders held firm. It also confirms that what scholars have found happening at the same time in parts of France and Germany happened also in Flanders. But one asks why such a huge book—a doctoral dissertation left intact—had to be written to substantiate what was done far better by Huizinga in a few chapters some thirty years ago. Even Toussaert asserts that the synthesis of Huizinga is well known, that it rests upon a sure analysis and is anchored in all the domains of the life and psychology of the late Middle Ages.

University of California, Berkeley BRYCE LYON

TRIBUNAUX ET GENS DE JUSTICE DANS LE BAILLIAGE DE SENLIS À FIN DU MOYEN ÂGE (VERS 1380–VERS 1550). By *Bernard Guenée*. [Publications de la Faculté des Lettres de l'Université de Strasbourg, Number 144.] (Paris: Société d'Éditions Les Belles Lettres. 1963. Pp. xii, 587.) This book provides an excellent example of the value of local history. By exploiting fully the archives of one *bailliage* the author gives a provocative account of the changes in legal administration and personnel between the late Middle Ages and the Renaissance in a broadly conceived manner reminiscent of Marc Bloch. Among his more challenging conclusions are that geographical boundaries were more stable and better known than Dupont-Ferrier believed. Jurisdictional quarrels were frequent because the division of responsibility between the various courts was not clear. These disputes rarely occurred between royal and seigneurial courts, but rather were more often between two courts of the same kind, for example, two royal courts. Revenue from high and low justice provided nobles with only a small percentage of their income so that the decline in seigneurial justice was not a severe financial concern. This decline resulted not from positive action by the crown, but rather from the litigants' preference for royal courts. The Châtelet court at Paris profited most by this trend because litigation there was more rapid and less expensive than at the parlement, but at the same time more just than at the *bailliage* court at Senlis. The number of judicial offices did not significantly increase between the reigns of Louis XI and Francis I, an interesting fact in view of the argument of some historians that the principal characteristic of the Renaissance monarchy was the increasing size of the bureaucracy. An important change did take place between 1490 and 1520 when the lieutenants of royal officials who had held their posts for limited periods were replaced by royal officials who held office for life, thereby reducing the opportunity of others to become officers. During the period of reconstruction following the Hundred Years' War it had been relatively easy for the outsider to become a judge, but by the reign of Francis I few besides nobles and the sons of royal officials could become office-holders. This did not prevent a fourfold increase in the number of *licenciés en lois*

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between 1500 and 1550, an increase that reduced the number of cases that came before each at the very time advance to royal officialdom was closed. One wonders whether this explains the lawyers' quest for municipal offices in the sixteenth century, a quest that brought them into conflict with the merchant oligarchy in many towns. Thus this excellent book undermines old clichés and raises new problems.

Emory University J. Russell Major

Modern

UNITED KINGDOM AND IRELAND

A TOWN GRAMMAR SCHOOL THROUGH SIX CENTURIES: A HISTORY OF HULL GRAMMAR SCHOOL AGAINST ITS LOCAL BACKGROUND. By *John Lawson.* (New York: Oxford University Press for the University of Hull. 1963. Pp. 302. $5.60.) Hull Grammar School has an unbroken history dating probably from the fourteenth and certainly from the fifteenth century. Its records are reasonably continuous, and Mr. Lawson has been able to write an account that gives us an adequately rounded picture of the school through its very long life down to the present generation. His scholarship is thorough and intelligent, but the material is by no means of equal interest. Much, particularly of the earlier part, is concerned with persons largely of interest to the local Hull historian. Information on curriculum and on the actual conduct of the school will, however, be generally useful. The most valuable part of the book is probably that dealing with the period since the late eighteenth century. The grammar school's neglect and decay after 1800 are a sad, revealing episode in general social history. The citizens of Hull could hardly have cared less either about their ancient but poorly endowed grammar school or about secondary (or higher) education for their children. It was only pressure from the central government that forced the civic authorities to provide, at the beginning of the present century, facilities for secondary education adequate for university entrance. The book would be even more interesting if the whole history of education in Hull since the end of the eighteenth century were more fully developed. Hull's grammar school was undistinguished throughout its long career, yet its uneventful and commonplace history in a middling seaport city is a valuable study of a central theme of English social history.

Haverford College Wallace T. MacCaffrey

ROGER ASCHAM. By *Lawrence V. Ryan.* (Stanford, Calif.: Stanford University Press. 1963. Pp. viii, 352. $7.50.) Roger Ascham is not a historical figure who improves with acquaintance; indeed, the more one knows of his work the less important it seems. He was one of Tudor England's greatest Greek scholars, but never obtained a professorship or published a classical text. He was tutor to Princess Elizabeth, but for less than two years. He was Latin secretary to both Tudor Queens, but had little influence on their policy and obtained scant reward for his service. He traveled in Germany as secretary to the English ambassador at the court of Charles V, but failed to see the continental humanists he most wished to meet. His works, *The Scholemaster, Toxophilus,* and *A Report of Germany,* prove on examination to contain the usual humanistic clichés but few fresh ideas. It is, nonetheless, good to have a careful examination of Ascham's career, as much to dispel the old image as to create a more nearly correct new one. Ryan moves with ease from the historical narrative of Ascham's life to the literary analysis of his works, utilizing all the relevant sources for both. Perhaps his most important contribution lies in rescuing from undeserved oblivion the *Report of Germany,* an interesting if naïve addition to the slender shelf of Tudor historiography. The

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serious reader will be irritated by the grouping of most notes at the end of the book, although the seemingly less important citations to Ascham's own works are given in parentheses in the main text. To add to the needless complexity, six notes dealing with bibliographical matters appear where they should: at the foot of the page.

University of Texas STANFORD E. LEHMBERG

SIR HENRY KILLIGREW: ELIZABETHAN SOLDIER AND DIPLOMAT. By *Amos C. Miller*. With a foreword by *A. L. Rowse*. ([Leicester:] Leicester University Press. 1963. Pp. xi, 279. 35*s*.) This is a painstaking and thorough biography of a "typical Elizabethan," as A. L. Rowse describes Sir Henry Killigrew in the foreword. A Cornishman, whose family controlled Pendennis Castle and saw some of its members become pirates, some civil servants, Sir Henry was a representative example of some of our favorite generalizations about the Elizabethan court gentry: how they lived on the perquisites of minor office and expanded their holdings of land by faithful service as ambassadors or civil servants. But it is Killigrew in all his uniqueness as Elizabeth's busiest ambassador that interests Miller, as it would have interested the late Conyers Read, who suggested the subject. Elizabeth never admitted him to her inner councils—whether because of his Puritanism, his "dullness," or his short stature ("Little Hal Killigrew," Leicester called him)—but he became one of her best diplomatic agents, in France, Germany, and Scotland. We do not learn anything startlingly new from the sources, printed or unprinted, about the negotiations in which he was involved, but in some cases, as in his several missions to Scotland, the picture is given in detail. Elizabeth seems to have thought of him as a very good second-rater. His Puritan zeal sometimes outran his patriotism, and his devotion to the Dudley family never wavered —other reasons for Elizabeth's suspicion. But he was closest in temperament, Miller feels, to Burghley, his brother-in-law. Occasionally the author succumbs to the biographer's temptation to tell us what Killigrew "perhaps" or "undoubtedly" thought or felt on some matter on which the sources are silent. And once he apparently uses the Elizabethan caricature in place of the real Machiavelli. These are minor blemishes in an exhaustive study and successful re-creation of a minor actor in the Elizabethan drama.

Princeton University E. HARRIS HARBISON

FOUNDATIONS OF ENGLISH ADMINISTRATIVE LAW: CERTIORARI AND MANDAMUS IN THE SEVENTEENTH CENTURY. By *Edith G. Henderson*. [Publication of the Ames Foundation.] (Cambridge, Mass.: Harvard University Press. 1963. Pp. 204. $5.00.) That use of the extraordinary writs of mandamus and certiorari was an important means of exerting judicial control over the acts of public officers has long been recognized by students of administrative law. (Mandamus, as its name indicates, is a command requiring the person to whom it is addressed to take a particular action. Certiorari is a writ that brings before a higher court the record of proceedings in an inferior tribunal.) The author of this study traces the development of those writs in England during the years 1600–1750. A writ of mandamus ordering restitution of a bailiff to his office in 1606 indicates that the basis of jurisdiction of the Court of King's Bench was found in a recital that the bailiff was arrested and detained "as he was coming to Our court of King's Bench . . . to confer with his counsel" concerning an action of debt then pending in that court. In other words there is a similarity to the privilege protecting members of Parliament from arrest when attending sessions, or to the rule established by American federal courts that a corporation cannot lawfully be excluded by a state from doing intrastate business because it resorts to a federal court in the exercise of a privilege to do so granted by the Constitution and laws of the

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United States. The remedy of mandamus was broadened by Lord Coke in James Bagge's case in 1615, which was often cited in later litigation. Subsequently a comprehensive modern theory of mandamus was first articulated by Lord Mansfield in 1762, when he said that "it ought to be used upon all Occasions where the Law has established no Specific Remedy, and where in Justice and Good Government there *ought* to be one." Certiorari in administrative cases first began to be used in the period 1635–1680 (although the writ itself was of ancient origin) in connection with the activities of commissioners of sewers (that is, drainage ditches). Not until about 1720 was it realized that not every requirement contained in a statute establishing a new agency was jurisdictional. Where "jurisdiction" existed, review on certiorari brought no relief because the propriety of the exercise of granted power was not reviewable by the court upon certiorari. This principle, which McIlwain shows to have existed in medieval law (see his *Constitutionalism Ancient and Modern,* pp. 77–78, 84–87), became a fundamental doctrine with respect to the scope of judicial review of administrative action.

Pittsburgh, Pennsylvania EDWARD DUMBAULD

CROSSROADS OF POWER: ESSAYS ON EIGHTEENTH-CENTURY ENGLAND. By *Sir Lewis Namier.* [The Collected Essays of Sir Lewis Namier, Volume II.] (New York: Macmillan Company. 1962. Pp. viii, 234. $5.00.) This second volume of his "Collected Essays" contains most of the late Sir Lewis' shorter pieces on English history, all except one previously published. Brought together, they add depth to that picture of England's eighteenth-century "mixed government"—partly royal, partly parliamentary—which was his chief contribution; they remind us also of his mastery of the historical essay. Others have studied the same sources, but few have been as insistent as Namier upon the importance of men's characters in determining their attitudes and behavior, or as successful in finding the phrase or the incident that reveals that character and explains its impact. Intolerant of weakness or pretense, he is yet generous in estimating the whole man (see his George III, his Townshend, or his touching Chesterfield). He has a gift, too, for presenting the universal in terms of the particular. The final piece in this volume, "Monarchy and the Party System," employs a logical rather than a chronological approach to show the transition of the eighteenth-century system to constitutional monarchy: this is Namier in essence—a brilliant summation of his life's work.

City College of New York DAVID I. GAINES

CITIZENSHIP AND CONSCIENCE: A STUDY IN THE THEORY AND PRACTICE OF RELIGIOUS TOLERATION IN ENGLAND DURING THE EIGHTEENTH CENTURY. By *Richard Burgess Barlow.* (Philadelphia: University of Pennsylvania Press. 1962. Pp. 348. $7.00.) This well-written and well-documented study deepens knowledge of the subject it treats though it hardly changes basic views. It is a study in why something did not happen. Between 1718 and 1829 religious liberty in England remained substantially on dead center, the Test and Corporation Acts subjecting Nonconformists to political disabilities they were easily able to remove in practice if they could bend their consciences a little. Why were *sub rosa* religious liberty and equality not made official? Why for more than a century did England prefer to circumvent the Test and Corporation laws rather than repeal them? Barlow might perhaps have made it clearer that the basic reason for this was fear of alienating a quiescent but potentially disruptive Tory Anglicanism and thus upsetting the political balance. The 1689 "settlement" was so successful that Englishmen had no desire to change it. It rested on toleration but also on the Church of England, a socially useful and politically necessary institution as Bishop Warburton demonstrated in 1736. Expediency, not princi-

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ple, was the rule of eighteenth-century British politics. Moreover the Dissenters, now increasingly prosperous and respectable, were anxious to live down their reputation as revolutionaries, and, in practice bothered not at all by the purely theoretical discriminations against them unless of exceptionally tender conscience, showed little enthusiasm for movements for repeal of the offending acts. This, too, the author might have stressed more: neither in the 1730's nor the 1770's did the majority of Dissenters offer much support to a crusading minority. Nor were they united. So far as concerned principle, the Dissenters, who wished full equality for themselves, were the fiercest foes of granting it to those who suffered much more than themselves from discrimination: the Roman Catholics. Thus it was difficult for most of them to stand foursquare for the principle of religious liberty and equality. Finally, just as the movement for repeal was picking up speed, the French Revolution and the enthusiasm of Dissenters like Richard Price and Joseph Priestley for it caused a reaction against the crusading liberalism of this group and all they stood for. Not until the wars of the French Revolution had died away could their principles triumph, though they sensed in 1770 that history was on their side. In his conclusion the author urges that their agitation was not in vain, despite its failure. He has attractively presented this interesting if somewhat negative story, digging up a few little-known facts as, for example, the 1779 relief to Dissenting schoolmasters in regard to the Thirty-nine Articles and the gaps in the yearly indemnification to Dissenters under the Test and Corporation Acts.

University of Maryland ROLAND N. STROMBERG

ADDITIONAL GRENVILLE PAPERS, 1763–1765. Edited, with an introduction by *John R. G. Tomlinson*. (New York: Barnes and Noble. 1962. Pp. ix, 347. $10.00.) The avowed purpose of this book is to supplement the four volumes of *The Grenville Papers* edited by W. J. Smith and published in 1852–1853 by John Murray. About three hundred pages of letters and over twenty pages of cabinet minutes are here published for the first time. All of these cover the period from 1763 to 1765 when George Grenville was First Lord of the Treasury. The value of this work lies not only in being a well-edited supplement to the four volumes of Smith, but also in the fine introduction which sketches the present location of nearly all of the material, both published and unpublished, available to Smith in 1852–1853. The editor lists the papers that were stored at Stowe and Buckingham House and were sold to Murray in 1851. Today most of these papers, published and unpublished, which were available to Smith, are still in the possession of Sir John Murray or have been acquired by the William L. Clements Library, Mr. W. S. Lewis of Farmington, Connecticut, or by the British Museum. Over a half million items of the Stowe MSS not sold to Murray were acquired in 1925 by the Huntington Library. Both the information in the introduction and the new material emphasize why this and other works of its kind make the task of the research scholar of the present easier than it was a generation ago.

Western Reserve University DONALD GROVE BARNES

CHARLES GRANT AND BRITISH RULE IN INDIA. By *Ainslie Thomas Embree*. [Columbia Studies in the Social Sciences, Number 606.] (New York: Columbia University Press. 1962. Pp. 320. $6.00.) This is a sound and competent study of a man whose career is intimately connected with the growth of British power in India during its most formative period, 1768–1823. Throughout his long connection with the Indian government, first as a company servant in Bengal, and then as a member of the court of directors in London, Charles Grant played an important role in all the varied controversies that marked the evolution of a trading company into the administrators of an empire. In this book his activities both in Bengal and in London are described in con-

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siderable detail and on the basis of extensive scholarly research. The author's central concern is to relate Grant's life to the larger currents of Indian reform that were stirring English society at the time. As a result the work is organized around an examination of Grant's views on three major questions: the company's monopoly, Christian missions, and territorial expansion. The first part of the book, dealing with Grant's career in India, is much the weakest, for there are few contemporary sources of information on this subject and little real evidence regarding Grant's influence upon Philip Francis, John Shore, and Lord Cornwallis. The author often falls back upon later reminiscences, especially in the chapter on the Hastings era, among them Grant's speeches in the court of directors, which he accepts uncritically as accurate expressions of Grant's earlier opinions. He also ascribes to Grant, on the basis of internal evidence alone, many documents written by his superiors. Embree at times thus falls prey to the occupational vice of so many biographers, that of overinflating the importance of his subject and standing forth as counsel for the defense. After Grant's election to the court of directors, his central role in the affairs of the East India Company is beyond dispute. And the author describes in fascinating detail Grant's advocacy of moral and social reform while at the same time obstinately defending the company's trading monopoly. Grant was perhaps unique among early nineteenth-century reformers in trying to keep apart those two movements—evangelicalism and free trade—which were in time to undermine the traditional structure of the company's government. On this ground, if no other, Grant's opinions clearly warrant the attention given to them in this book.

University of California, Berkeley THOMAS R. METCALF

TRADE UNION GROWTH, STRUCTURE AND POLICY: A COMPARATIVE STUDY OF THE COTTON UNIONS IN ENGLAND. By *H. A. Turner*. (Toronto: University of Toronto Press. 1962. Pp. 412. $7.50.) To emphasize the importance of the cotton textile industry to the growth of industrial Britain is almost a commonplace. To emphasize, however, the importance of the cotton textile unions to the industry is interesting indeed. This volume, the first major analysis of the English cotton unions since the work of the Webbs, is significant, for it not only traces the growth of the unions within the industry, but it also analyzes the role of these unions within the British trade-union movement as a whole. Turner divides his work into a series of "books" of two related chapters each. The first is a general introduction emphasizing the geography of the industry. In the second he discusses the origins of the cotton unions and demonstrates that they are, using his definitions, far older than is often suggested. The third book is concerned with the type of unions that developed within the industry in the mid-nineteenth century. For example, while the spinners retained the craft union, restricting membership, the weavers were early to adopt the so-called "New Model" or open union which embraced a far less restricted segment of the industry. The latter, the author argues, set the pattern for large modern industrial unionism. In the fourth book, Turner compares the structure of these cotton unions with that of other contemporary British unions, explaining the reasons for the predominance of the craft union in this period. He then shifts his attention to the internal structure of modern trade-unions in Britain and concludes that "trade union forms are a product of trade union origins." In his fifth book, Turner is concerned with a multitude of problems involving the large industrial unions that developed during the 1890's. The tendency of large unions to fragment, the creation of an elite within a union, the strengths of the federal structure of the cotton unions, the extent to which the membership of a union can control policy, and other similar problems are discussed here. In his sixth and final book, Turner turns his attention to trade-unionism from 1914 to the present. He is especially interested in analyzing, of course, the cotton textile unions,

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particularly their aims and policies, their connection with the Labour party, their dwindling membership, and the like, during this, the period of sharp decline of the cotton textile industry in Britain. His observations are astute and in places highly critical of the trade-union movement. Although Turner often protests that he is not a historian, his careful use of trade-union sources and his historical approach to the modern textile unions make this a work of real significance to historians of modern British economic history. His clear style and trenchant comments add much to the understanding of a frequently difficult subject.

University of South Carolina CHARLES W. COOLIDGE

ORIGIN AND HISTORY OF CHRISTIAN SOCIALISM, 1848–54. By *Torben Christensen*. [Acta Theologica Danica, Volume III.] (Aarhus: Universitetsforlaget. 1962. Pp. 369.) How often a topic, a period, a man is talked about, referred to, in terms as meaningless as a conversation two doors away. The words are familiar, but they add up to nothing. Thus has it been with Christian Socialism. Conventionally reported as a blend of social sweetness and light, vaguely influential, vaguely related to Chartism, vaguely tractarian, teachers as well as authors label it another sample of Victorian humanitarianism and hasten on to something more concrete. Indeed it is very nearly a case of F. D. Maurice and Charles Kingsley, Thomas Hughes, and, less commonly, J. M. Ludlow, and all that sort of thing, which sort of thing on examination often turns out to be quite erroneous. If one has the patience and concentration he can get a much more substantial impression of the movement from this account which the professor of ecclesiastical history at the University of Copenhagen has based on tracts, periodicals, and manuscripts. Patience and concentration it will take because of the detailed, almost tortuous exploration of the various objectives and diverse opinions of the individual Christian Socialists. Like so many movements it was not one but many. It was propagandist; it was fact finding; it was activist. It promoted workingmen's associations, sanitary reform, cooperatives, and a workingmen's college. Not all Christian Socialists subscribed to all these tactics and aims, and thereupon followed the "winding-up" of the movement. Yet disagreement in policy must not obscure the unanimous concern with the "condition of England" question. Nor must it obscure the generally practical outlook. "It was characteristic of the Christian Socialists," says Christensen, "that they did not give themselves up to romantic dreams of bringing back pre-industrial England—only in Kingsley [who is no hero to our author] can we find this tendency. They accepted the industrial revolution and unhesitatingly acknowledged the benefits it might confer on mankind." In this respect Christian Socialism contrasted sharply with the "Heavens Below" projects and with later Owenism and even Chartism.

University of Missouri CHARLES F. MULLETT

THE ACTON–NEWMAN RELATIONS: THE DILEMMA OF CHRISTIAN LIBERALISM. By *Hugh A. MacDougall, O.M.I.* (New York: Fordham University Press. 1962. Pp. xi, 199. $5.00.) It has long been known that an interesting relationship existed between Lord Acton and Cardinal Newman and that the latter, who was older by thirty-three years, had been involved in the juvenile journalistic enterprises of the former, and, like him, had been perturbed by the movement for the formal declaration of papal infallibility. It has also been known that the two men differed and that in 1859–1860 Newman had been favorable to Napoleon III and the Italian movement, hostile to Austria, and ready to see the abandonment of the pope's temporal power, while Acton was still for a time passionately on the opposite side in these matters. Also Newman did not show the recalcitrancy on the subject of papal infallibility that made Acton so remarkable in the period before and after 1870. Furthermore, the historian,

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particularly in the latter part of his life, made some very severe judgments on the cardinal. MacDougall has set out to study in detail the dramatic relationship between these two men, using, particularly, a wider range of Acton papers than has hitherto been possible and also the Newman papers at the Oratory, Edgbaston, and the Blennerhasset Papers acquired a few years ago by the Cambridge University Library. By careful dovetailing he has produced a more comprehensive view of Acton's journalistic activities in the late 1850's and the 1860's than has hitherto been available. He has examined the main problems: the attitudes of Acton and Newman (and the interrelations between them) in respect to the temporal power and the Vatican Council, and the important section of Acton's thought which deals with Newman's idea of Development. He has new things to say about Acton's acceptance of the infallibility decree and about the interesting influence of the man on the construction of Newman's *Apologia.* The whole subject is one on which it was important to have a contribution by a Catholic mind, and MacDougall has cleared away a number of errors and misunderstandings. He has shown the curious power that secular liberalism acquired over the very Catholicism of Acton, but he has not overlooked the ultimate sincerity of Acton's piety, as revealed in the most private records that survive. The status that the historian gave to secular liberalism goes far toward explaining his alienation from Newman. In two respects this careful and interesting book, which brings many new side lights, shows the hand of a comparatively young writer; perhaps we can hope that the author will sometime make good the defect. The work would gain much if something more like a portrait of both Acton and Newman emerged from it. And the final chapter, which raises the profoundest issues of all—the really fundamental difference between the views of the two men on progress, history, and politics—though it sets out the materials in an interesting way, leaves us calling for a still higher, synthetic treatment. MacDougall ought some day to provide us with a more comprehensive and philosophical survey of what lies behind the two different outlooks.

Peterhouse, Cambridge, England H. Butterfield

THE ELIMINATION OF CORRUPT PRACTICES IN BRITISH ELECTIONS, 1868–1911. By *Cornelius O'Leary.* (New York: Oxford University Press. 1962. Pp. 253. $5.60.) Increasingly, study is being directed to the problem of political corruption in Britain. Attention has shifted in recent studies from reform of the franchise and of the size of electoral districts to the problem, in the nineteenth century, of the costs of the election process and of corrupt electoral practices in the constituencies. This trend is clearly observable in the works of such writers as Norman Gash, G. Kitson Clark, and H. J. Hanham, who, in dealing with the broader problems of political parties and their election techniques, are concerned also with the persisting problem of electoral corruption. This corruption in the constituencies had been so generally accepted that even such an outstanding Victorian as Walter Bagehot was forced to conform. O'Leary's book concentrates on the legislation by Parliament in the period 1868–1911, with special emphasis on the statute of 1883, the Corrupt and Illegal Practices Act, designed to eradicate electoral corruption. It poses two questions: "1. What caused the change in the attitude of Parliament to these practices? 2. How did the politicians manage to get their reformed ideas across to the public?" In seeking to answer these questions he has studied the evidence in the Parliamentary Papers and contemporary journals and newspapers. True, as he admits, it is "essentially an essay in political pathology." His conclusions, unlike those of Gwyn in a recent study entitled *Democracy and the Cost of Politics,* indicate that "Democratic theory played only a marginal part in this great change." O'Leary is convinced that the excessive costs of the general election of 1880 and the exposures of corruption by the petition courts and royal commissions "raised an immediate

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reaction from the newspapers and members on both sides of the House of Commons and that reaction more than any other factor was responsible for the carrying through in 1883 of a draconian limitation of expenses which insured that the next general election would be just over one-quarter as expensive as the election of 1880." This was the turning point, and O'Leary concludes that the national party managers and the M.P.'s cooperated in making this legislation work. This is a careful and thoughtful book.

Brooklyn College MADELINE R. ROBINTON

JOHN ANDERSON, VISCOUNT WAVERLEY. By *John W. Wheeler-Bennett.* (New York: St Martin's Press. 1962. Pp. xv, 430. $10.00.) This is a detailed, lucid, useful, and curiously flavored biography of an important British public figure. Son of an Edinburgh shopkeeper, John Anderson (1882–1958) began winning prizes at a Scots day school. A brilliant university career in science prefaced his 1905 entry, "top of the list," into the civil service. Choosing the Colonial Office, he rose quickly, distinguishing himself as Undersecretary at Dublin Castle until after the 1922 treaty when for a decade he was Permanent Undersecretary of the Home Office. Anderson thus joined a select group, including Hankey, Secretary to the cabinet, Fisher, head of the Treasury, and Vansittart, Permanent Undersecretary of the Foreign Office, who were largely responsible for the smooth functioning of the machinery of the British government. In 1932 he left the service and for five years governed Bengal efficiently and, as circumstances demanded, courageously. Entering Parliament as a nonparty member, he became Chamberlain's Privy Seal in 1938, with special responsibilities for civil defense. Under Churchill he was in turn Home Secretary, Lord President, and Chancellor of the Exchequer. While in opposition he accepted from Attlee the chairmanship of the UK advisory committee on atomic energy, helping to negotiate the abortive postwar Anglo-Canadian-American accord and supervising Britain's independent atomic program. He also headed the Port of London Authority, held numerous remunerative directorships, and accepted many honors including elevation to the Lords in 1952. Acknowledging his outstanding state services, Queen Elizabeth had the Order of Merit conveyed to his hospital bed as he lay dying. Wheeler-Bennett's volume contains essential new material on British internal politics, and he gracefully depicts the extraordinary career of an administrative genius whom he deeply admires. Owing, however, to his biographer's complete frankness, Viscount Waverley sometimes appears to be little more than a supremely successful careerist—and humorless at that. Though the author calls him "a giant among men," some readers will wonder if one had to know Viscount Waverley to appreciate him fully.

University of South Carolina GEORGE CURRY

THE ALEXANDER MEMOIRS, 1940–1945. By *Field-Marshal Earl Alexander of Tunis.* Edited by *John North.* (New York: McGraw-Hill Book Company. [1962.] Pp. xiii, 209. $5.95.) This is a rather innocuous book. It cost the author but little labor—chiefly some reworking of his dispatches—and it offers the reader very little that is not to be found elsewhere including a few misstatements of fact. The arrangement is rather puzzling, for the brief accounts of Dunkirk (1940) and of Burma (1942) are sandwiched in between the comments on North Africa and Italy. The pictures are nice, and the maps are appropriate. Possibly the best parts are the characterizations of Churchill, Montgomery, Eisenhower, Patton, and other leading figures of the Allied camp, but even here one senses a restraint better calculated to maintain friendships than to enlighten posterity. Alexander touches on several controversial problems, but resolves none of them. He regrets that in the summer of 1944 seven divisions were taken from his command in order to seize Marseilles, thus halting the momentum of his advance and

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eliminating the chances for an invasion of the Danube Valley. He blames General Lucas for being too slow and cautious at Anzio. His brief remarks about Sicily fail even to suggest that he himself missed a splendid opportunity to destroy the opposing German forces. No information whatsoever is given on the Allied failure to cut off the German Tenth Army after the breakout from Anzio in June 1944.

Alexandria, Virginia HOWARD M. SMYTH

BRITISH FOREIGN POLICY: THE PROCESS OF READJUSTMENT, 1945–1961. By *F. S. Northedge*. [Minerva Series of Students Handbooks, Number 7.] (New York: Frederick A. Praeger. 1962. Pp. 341. $6.50.) For a historian a book such as this must be regarded as no more than an interim report. Essentially it is a clear and proportioned account of Britain's responses and actions in the long series of perplexing and frustrating situations facing it in the postwar period; as such it is successful and illuminating. Mr. Northedge has no thesis to uphold, no "scientific analysis" to offer; he is concerned primarily with the course of diplomacy as it can be demonstrated from the published documents. Comment and criticism, though by no means missing, are deliberately subordinated to explication. This volume will be more useful for reference than the broader and more discursive work of C. M. Woodhouse, *British Foreign Policy Since the Second World War* (see *AHR*, LXVIII [Jan. 1963], 514). The basic theme in the story is the impossibility, pretty suddenly manifest in 1945, that Britain should continue to have a genuinely independent foreign policy. Material impoverishment, the decline of Europe, the rise of the superpowers, the new technology of war, all forbade it. "Diplomacy on a shoe-string," as the author calls it, was imposed on the former Great Power, and nothing he says indicates any great change down to 1961. Britain was still in the control room of world politics, but its hand could no longer pull the levers. The great demonstration of this at Suez in 1956 is unsparingly told. Yet on the whole the process of readjustment was intelligently made, and Northedge's careful accounts of British policy in such matters as Germany, Palestine, and arms control will help students to keep the record straight. What he does not do should, however, be noted. Defense policy and the British role at the United Nations are deliberately omitted; colonies and Commonwealth figure only in connection with the application to enter the Common Market. On the other hand, the overshadowing presence of the United States is almost everywhere. The author draws out very clearly the contrast—most marked, but not exclusively in the Dulles era—between the British tendency always to search for accommodation and the American conception of politics as a confrontation of good and evil. The reader is left with a good understanding of the rooted characteristics of British thought and opinion and a renewed conviction that America is indeed hard to understand.

Clark University H. DONALDSON JORDAN

IRISH FEDERALISM IN THE 1870's: A STUDY IN CONSERVATIVE NATIONALISM. By *Lawrence J. McCaffrey*. [Transactions of the American Philosophical Society, New Series, Volume LII, Part 6.] (Philadelphia: the Society. 1962. Pp. 58. $1.50.) This study is an important addition to Irish parliamentary history under the union. It sets out to show that the Home Rule movement under Isaac Butt was more than an unimpressive prelude to the era of Parnell. Historians, the author suggests, have underestimated Butt's success in arousing Irish nationalist feeling after the earlier failures of repeal, tenant right, and Fenianism. He argues, furthermore, that insufficient attention has been given to Butt's objectives, related as they were to his political conservatism. Butt wanted a self-governing Ireland sharing in the benefits of the British constitution and Empire, as well as reform which would give stability to Irish

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society; he wanted to avoid alliances with British radicalism and secularism. In developing his thesis Mr. McCaffrey studies the election of 1874, the Home Rule party and Irish public opinion, and those organs of action and opinion outside Parliament: the Home Rule League in Ireland, and the Home Rule Confederation of Great Britain. A final chapter deals with the crisis in the party's history caused by the Parnell-Biggar policy of obstruction. In concluding, the author does not deny that Butt's character and personality had something to do with his failures of leadership. He does argue, however, that many of the Irish members elected in 1874 were unpromising material, tentative in their dedication to federalism. Nor, in the long run, was Butt's attempt at a harmony of interests for the good of Ireland successful. "The gentry frowned on Butt's agrarian radicalism," writes McCaffrey, "and the tenant farmers refused to accept the separation of the Home Rule and tenant-right issues." In preparing this well-written study, McCaffrey has consulted the Butt manuscripts in the National Library of Ireland, Butt's numerous published writings, and many contemporary Irish newspapers. With Mr. J. H. Whyte's *The Independent Irish Party, 1850–59* (1958), the present work gives us another link in the history of the Irish membership in the House of Commons between O'Connell and Parnell.

Connecticut College HELEN F. MULVEY

EUROPE

POETS, PATRONS, AND PROFESSORS: SIR PHILIP SIDNEY, DANIEL ROGERS, AND THE LEIDEN HUMANISTS. By *J. A. Van Dorsten.* [Publications of the Sir Thomas Browne Institute, Leiden. General Series, Number 2.] (Leiden: University of Leiden Press; New York: Oxford University Press for the Institute. 1962. Pp. xi, 227. $6.10.) The Sir Thomas Browne Institute has set itself the admirable and difficult task of determining the breadth and depth of Anglo-Dutch cultural relations in the first century or so after the Dutch Revolt. Van Dorsten's book is the third such study to have been published: his book on the English printer in Leiden, Thomas Basson, appeared in 1961; in 1962 the first volume of Bachrach's study of that paradigm of Anglo-Dutch relations, Constantijn Huygens, came out. The present work deals, very interestingly, with the details of Daniel Rogers' connections with the Netherlands in general and in particular with the programs and personnel (religious and humanist) of the new university at Leiden. A student of literature, Van Dorsten is more interested in the humanist program and its products than in the religious program: he examines Rogers' contributions to the work of various Neo-Latinists whom he dubs "the Leiden school." (They lived in Leiden: their Latin is not distinguishable, for better or worse, by their place of residence.) Through these men, Van Dorsten makes even more relevant than they have been the Dutch connections of that English Protestant nonpareil, Sir Philip Sidney. We are given a view from Leiden, so to speak, of Sidney, of Leicester, of Geoffrey Whitney (the emblem writer), and we are invited to consider Leiden's part in the creation of the "Sidney myth," to which end there is an appendix with the text of forty Latin poems about Sidney. Unquestionably the product of scrupulous scholarship, the book informs us much about the precise arrangements of Anglo-Dutch cultural commerce, without which one cannot understand the larger issues. Van Dorsten knows his way about the complicated period he describes; his excellent English makes it appear quite clear and manageable. All the same, one is left a bit disappointed. Why? Because this is a subject too great to be weighed out in scruples only; we require not just a meticulously documented narrative, but a matured cultural evaluation as well. The patron saint of these studies, Sir Thomas Browne, said he loved the small more than

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the great, but his prose too often suffered a lack of proportion because of his preoccupation with detail. Another Leidenaar, Johan Huizinga, might serve as a subpatron to the institute's authors: if this series could make precise investigations serve, as in Huizinga's work it did, a more sweeping cultural interpretation than has so far been ventured, it would carry on the tradition of cultural history that is Leiden's proud burden.

State University of Iowa R. L. COLIE

FROM STOCKHOLM TO ST. PETERSBURG: COMMERCIAL FACTORS IN THE POLITICAL RELATIONS BETWEEN ENGLAND AND SWEDEN, 1675–1700. By *Sven-Erik Åström*. [Studia Historica, Number 2.] (Helsinki: Finnish Historical Society. 1962. Pp. 146. $2.50.) After so many excellent studies of Baltic economic affairs by Finnish scholars (Mickwitz, Niitemaa, Jutikkala), Åström's book comes as a disappointment. Title, subtitle, and dates in the title are misleading. Indeed, only one short chapter, with the same title as the book, covers the topic and, at that, deals with a different century (1699–1747). The English translation in which the original Swedish manuscript is published is inadequate. Incorrect factual statements include the assertion that Narva in 1553 was in Russian [!] hands and that the Continental System was responsible for a revision of the English-Swedish treaty in 1803 [*sic*]. It is stated that after 1617 Narva became a matter of international politics when actually its international significance for more than a century has often been demonstrated, as recently as in Volume CLXXII (1952) of the *Historische Zeitschrift;* that Sweden's conquest of Livonia "cut off" Riga, Reval, and Narva from their hinterland when, to the contrary, the traditional connections of Livonia, which had never formed one political unit with its hinterland, were maintained; that in 1697 Russian tar "made its entrance" into Western European markets although it had played a role there for centuries. Moreover, it was not Erik XIV who conceived the idea of developing Elfsborg as a staple place for the Russian trade but Gustavus Vasa, and even Sten Sture before him. The reader can profit from the author's discussion of English interest in substituting, as early as 1700, America for Sweden as a producer of naval stores, and from his discussion of England's reaction to Sweden's monopolistic endeavors and England's policy of *divide et impera* and the Baltic balance of power. The description of the Swedish Tar Company is useful but not comprehensive enough. With a more exhaustive use of available source material (the author confined himself mainly to that from England), one or two good articles, but not a book, might have been written, and broader analyses of Swedish-English relationships might have been made than those attempted. The two appendixes on copper and brass and on tar and the various added statistics could also furnish material for an interesting article, but again one concerned with a period different from that presumably treated in the book.

University of Delaware WALTHER KIRCHNER

DEUTSCHER UND BRITISCHER PARLAMENTARISMUS: EIN VERFASSUNGSGESCHICHTLICHER VERGLEICH. By *Gerhard A. Ritter*. [Recht und Staat in Geschichte und Gegenwart, Number 242/243.] (Tübingen: J. C. B. Mohr (Paul Siebeck). 1962. Pp. 56. DM 4.50.) In this expansion of his *Antrittsvorlesung* Ritter compares the parliamentary institutions of modern Britain with those prevailing in Germany under the Weimar Republic. He ascribes the failure of the German parliamentary experience not merely to constitutional defects but also, and more, to the historically determined character of German political traditions and to the lack of practical experience in developing a party system. He does not pretend to present the results of new research,

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but makes intelligent use of the monographic literature, including some of the most recent studies which have greatly altered our perspective on these problems.

State University of Iowa WILLIAM O. AYDELOTTE

DIE FINNISCHEN EISENBAHNEN IN DEN MILITÄRISCHEN UND POLITISCHEN PLÄNEN RUSSLANDS VOR DEM ERSTEN WELTKRIEG. By *Tuomo Polvinen*. [Studia Historica, Number 4.] (Helsinki: Finnischen Historischen Gesellschaft. 1962. Pp. 295. $3.50.) This academic dissertation was the subject of a model review by the Finnish historian Jaakko Suolahti in the *Historiallinen Aikakauskirja* (No. 2, 1962, 139–43). Perhaps it will suffice to summarize his conclusions. On the subject, he writes: "very skillfully chosen and richly productive." While narrow enough for detailed investigation, it nonetheless illuminates broad foreign policy issues, notably Finnish-Russian relations from the perspective of Russian security, defense, and political interests. Concerning the sources, he says they are "completely adequate . . . ably and critically used." Polvinen has worked in the archives at Moscow, Leningrad, Stockholm, and Helsinki; his list of printed works abounds with Russian-language studies. He comments that the contents are "logically and well organized." Eight chapters deal with such topics as the significance of railroads in an imperialistic epoch, Russian defense preparations, Governor General Bobrikov's railroad policies, and Swedish reactions; the longest chapter, running to 143 pages, examines in detail Russian railroad politics, 1905–1914. Especially illuminating are the accounts of how Finnish-Russian relations in this sector were influenced by such events as the Russo-Japanese War and the German naval expansion. Finally, concerning style, Suolahti writes: "enjoyable and lively."

Heidelberg College JOHN I. KOLEHMAINEN

DIE STELLUNGNAHME DER SOWJETUNION GEGEN DIE BESETZUNG DES RUHRGEBIETES: ZUR GESCHICHTE DER DEUTSCH-SOWJETISCHEN BEZIEHUNGEN VON JANUAR BIS SEPTEMBER 1923. By *Wolfgang Ruge*. [Deutsche Akademie der Wissenschaften zu Berlin. Schriften des Instituts für Geschichte. First Series, Allgemeine und deutsche Geschichte, Number 12.] (Berlin: Akademie-Verlag. 1962. Pp. xv, 198. DM 23.) It is Dr. Ruge's thesis that Germany had but one friend during the French occupation of the Ruhr in 1923. That one friend, "the only state on earth which defended Germany's independence, sovereignty, and unity" in its hour of need, was the Soviet Union. It was entirely logical that this should be so, and that the representatives of "Soviet man" should take the part of Germany's true interests against "monopoly lords," "class enemies," "barons of coal and smokestack," "predatory imperialists," and "monopoly bourgeois": "The Soviet attitude toward the imperialistic policy of force pursued by France was not the result of any changing tactics but corresponded altogether to the over-all lines of Soviet foreign policy, a policy that recognizes the equality of rights of all nations, consistently acts for the maintenance of peace, stands by the principles of proletarian internationalism, and is based on the peaceful coexistence of states possessing different social systems." To support these contentions, Ruge cites an impressive amount of sources, both primary and secondary. If, in going over this wealth of material, any doubts ever occurred to him about the total and indisputable accuracy of the current party line, he does not share them with the reader.

Indiana University JOACHIM REMAK

LA VALLÉE DE CAMPAN: ÉTUDE DE SOCIOLOGIE RURALE. By *Henri Lefebvre*. [Bibliothèque de Sociologie contemporaine.] (Paris: Presses Universitaires de France. 1963. Pp. 220. 18 fr.) Campan is a rural community embracing a small valley in

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the Pyrenees, not far from the Spanish border. It has been in place a long, long time, and its history over the last thousand years is remarkably well documented. In this study, haphazardly put together and written in gnarled prose, the noted sociologist and Marxist philosopher Henri Lefebvre has reconstructed that history up to the Revolution. To be more precise, he has chained together the papers of the communal archives and a very small number of other sources with critical comments and speculations. The first third of the book offers a series of commented excerpts from documents and descriptions of Campan, the second third analyzes the basic structure of the community and its long struggle to maintain its independence, and the final section discusses local history from the end of the sixteenth century to 1800, as recorded in the voluminous deliberations of the community's council. In Lefebvre's hands, the documents form a paradigm of rural modernization. More or less together (he argues, in a series of asides) we witness the decline of primitive democracy, the development of internal divisions and hierarchies, the consolidation of private property, the increase of external involvements, the concentration of political power, and the subjugation of the community to the crown. There is much in this analysis that is both significant and convincing, even if important sections of the supporting historical account consist of conjecture and very liberal interpretations of the sources. The reader who works his way through this trying book is likely to be as impressed with the toughness and durability of mountain democracy as he is with its supposed enfeeblement before the Revolution.

Princeton University CHARLES TILLY

THE FRENCH SECRETARIES OF STATE IN THE AGE OF CATHERINE DE MEDICI. By *N. M. Sutherland*. [University of London Historical Studies, Number 10.] (London: University of London, the Athlone Press; distrib. by Oxford University Press, New York. 1962. Pp. xii, 344. $8.00.) Miss Sutherland's monograph deals with the twelve holders of the title Secretary of State under the French crown between 1547, when the first four were named by Henry II, and September 1588, when Henry II, with mystifying suddenness, dismissed all three incumbents. The mad King's impulsive disgrace of these officeholders did no more than interrupt the history of the office itself, as any student of the subsequent Bourbon monarchy is well aware. It did, however, end a stormy and important chapter. The years of Queen Catherine and the early stages of the religious war, in particular, revealed the subtle growth of power in the hands of the high bureaucracy, a growth which may have infuriated the last of the Valois, but which princely caprice, then and after, was unable to reverse. The author has some interesting pages on medieval origins at the beginning of her work and a reasonable examination of the crisis of 1588 at its end. In between, however, she presents a curiously immature treatment of her challenging subject. Countless pages are spent on genealogical details, often followed so far beyond the immediate event or appointment as to distract even the most patient reader. She is too much the chronicler in another respect as well, repeatedly accepting at face value the courtly rhetoric of the French Renaissance as though it represented genuine psychological perceptions. On page 156, for example, she says of Neufville de Villeroy—undeniably a considerable figure: "Even those who were not his friends admitted that he had outstanding qualities, 'une grande integrité espurée de toute avarice, une grande modestie, une exquise proprieté et une vigilance incroyable.'" The quotation is from Pierre Matthieu, Villeroy's biographer! The sixteenth-century Secretaries of State deserve careful treatment in terms of both institutional history and political sociology. Such a study, however, would demand the sophistication that comes only with sure perspective and with the ability to keep the topic in context. Meanwhile, what amounts to a cluster of biographies, interspersed

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with narrative vignettes, barely justifies either the writer's or the reader's expenditure of time.

Harvard University FRANKLIN L. FORD

DOCUMENTS RELATIFS À LA COMMUNAUTÉ VILLAGEOISE EN BOURGOGNE: DU MILIEU DU XVII[e] SIÈCLE À LA RÉVOLUTION. By *P. de Saint Jacob.* [Publications de l'Université de Dijon, Number 28.] (Paris: Société Les Belles Lettres. 1962. Pp. xxxii, 157.) These sixty selected documents supplement the late P. de Saint Jacob's prodigious archival study of the peasantry of northern Burgundy in the seventeenth and eighteenth centuries (*AHR,* LXVII [Oct. 1961], 118). The annotated documents are intended to illustrate rather than to prove systematically the author's main contention that the traditional village community with its collective rights and customs was forced to give way to the "agrarian individualism" represented by the new commercialized domain. The documents expose royal efforts to preserve village communal institutions after the wars of the Fronde and their ultimate failure in the eighteenth century. Most enlightening are the deliberations and protests of the local village assemblies regarding such problems as the apportionment of royal taxes, the abuses of clearings, the usurpation and division of communal land, the renewal of rent rolls (terriers), and the enforcement of crop rotations. In addition to supplementing the author's thesis, the documents add useful information about farm produce, land values, local justice, village officials, and rural living conditions. Sixty selected documents may be insufficient to place this publication in a class with the standard collection of economic documents of the French Revolution. They represent, however, much more than a few *pièces justificatives* for Saint Jacob's regional study; they are a printed source no social historian of the old regime should neglect.

Dartmouth College ROBERT FORSTER

ROUSSEAUS FREIHEITSLEHRE. By *Otto Vossler.* (Göttingen: Vandenhoeck & Ruprecht. 1963. Pp. 394. DM 28.) In this study, Vossler aligns himself quite openly, though without naïve simplicity, among the many defenders of Rousseau, both in his thought and in his historical significance. Even if Rousseau was not a systematizer, yet there is logic, as Vossler says, in the Genevan's development as a thinker. *Rousseaus Freiheitslehre* is an inquiry into the formation and fruition of Rousseau's ethical and political thought. The "whole Rousseau," Vossler commendably insists, must be studied in order to understand his writings on state and society. Likewise commendable is the author's primary reliance upon Rousseau's own works rather than upon the tangled mass of critical commentary that so often has virtually smothered them. Many readers of Vossler will, however, find cause for serious questioning, on both major and minor points. Does, for example, the author perhaps find more order and coherence in Rousseau's work than are really there? Is it indeed true, following Vossler's central contention, that Rousseau's political theory is essentially a complete negation of earlier political thought, a basically new construction which happens at times to employ the older phraseology? Rousseau, Vossler maintains, rejected completely the abstract, rational political theory of his predecessors and built his state and his society upon man himself and man's freedom; Rousseau "humanized" the state. Certainly one will agree with Vossler that for Rousseau, by and large, abstract reason was replaced by feeling, by impulse, by will, but was there perhaps in Rousseau more of the rationalist than Vossler concedes? In any case, though inevitably Vossler often traverses well-traveled ground, and though one may now and again question his judgment, Rousseau's doctrine of human and social freedom here receives well-organized, stimulating treatment,

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notable for its restoration of Rousseau's political theory to its place within a broader ethical context.

University of Akron HENRY S. VYVERBERG

CALONNE: FINANCIER, RÉFORMATEUR, CONTRE-RÉVOLUTIONNAIRE, 1734–1802. By *Robert Lacour-Gayet.* (Paris: Hachette. 1963. Pp. 510. 35 fr.) To a degree this biography partakes of the qualities of its subject, Charles-Alexandre de Calonne, intendant of the *généralités* of Metz and Lille from 1766 to 1783, controller general of finance from 1783 to 1787, and in effect first minister of Louis XVI's brothers during the early years of the emigration, 1790 to 1792. He was a charmer, a projector, a big dealer as much as a new dealer. He worked hard and rapidly, his views on general policy were usually perceptive and sound, and he presented them eloquently. The rapidity of his work, however, exposed him, not always unjustifiably, to the charge of superficiality. There was, not infrequently, a gap between his vision of general strategy and its detailed, tactical implementation. His biographer, similarly, has labored manfully. He has read the many cartons of manuscripts in the Archives Nationales and the Public Record Office, a vast range of memoirs, and key monographs. He has grasped correctly the essentials of Calonne's character and action in nearly every situation. He has a gift for eloquent evocation of the past. His characters speak and act as if they live once again. Nevertheless, the author may have moved a little too rapidly in the preparation of the book. My investigation has paralleled that of the biography in the chapter on Calonne's economic accomplishment as controller general. In the biography the characterization of Calonne's economic policy (neither mercantilism nor laissez faire, but a pragmatism of optimism) cannot be improved. The description of separate aspects of this policy is excellent. There are no citations, however, which might indicate that each phrase has been meticulously checked against the evidence. Some statements are misleading. To imply, for example, that the appointment of the chemist Berthollet as consultant to the Bureau of Commerce and the importation of English mechanics were due solely to Calonne's innovating spirit is to rob Calonne's predecessors and the bureau's civil servants of credit owing them. An occasional sentence is simply wrong. "Necker, solely preoccupied with credit operations, scarcely bothered himself [about economic policy]." Actually, Necker worked steadily at being his own director of commerce, and he had definite economic policies, though they were not those of Calonne. The biography, in many respects a superb work and especially valuable on the period of the emigration, suffers from its author's failure to apply to each of his sentences the simple question: is this true?

Duke University HAROLD T. PARKER

ZHAN-POL' MARAT I GRAZHDANSKAIA VOINA XVIII V. [Jean-Paul Marat and the Civil War of the Eighteenth Century]. By *Ts. Fridliand.* (2d ed.; Moscow: Publishing House of the Academy of Sciences. 1959. Pp. xv, 558.) Something of a mystery surrounds this book and its author. The late Zvi Fridliand, who devoted a large portion of his energies to the study of the life and ideas of Marat, published his first work on the "friend of the people" in 1926 (*Jean-Paul Marat before the Great French Revolution,* in Russian) and the present one in 1934. The latter was designed to be the first of two volumes entirely devoted to Marat's activities and the development of his revolutionary beliefs during the Revolution itself. The second volume never appeared; there are no explanation in the editor's preface to the second edition as to why it did not, no mention of the year of Fridliand's death, and no reference to him or his books in the article that the *Great Soviet Encyclopedia* devoted to Marat in 1954 (Vol. XXVI). These points are raised because they make one wonder whether the reissue of the

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work is an act of piety and respect for its author, its subject, or both. There was something of a gap in Soviet studies on the French Revolution and its antecedents between the mid-1930's and mid-1950's; the present work may have been designed to help fill it and to pay tribute to the fact that there was a pre-Marxist revolutionary tradition outside of Russia. This may also, possibly, have been one reason why the work was not completed. For it made of Marat such a consistent advocate of a revolutionary dictatorship based on the plebeian opposition in the Revolution that one is at times tempted to think that Fridliand was talking not of Marat but of Lenin. In fact, in the long introductory chapter to this volume (which goes up to only 1791), *Maratizm* is presented as a complete and consistent revolutionary doctrine that had its roots in Thomas Münzer, the Levellers, plebeian movements in eighteenth-century Switzerland, and a series of works that appeared in Europe before the French Revolution and were devoted to the strategy and tactics of the "armed rising." *Maratizm* is described not only as an ideological tendency but also as a definite, organized group of which Marat was the leader in the first years of the Revolution. It is questionable whether Marat was as consistently revolutionary as Fridliand makes out, especially before the Revolution; whether he went as far beyond the main body of Jacobinism and moved as close to the *enragés* (whose radicalism he on occasion opposed) as here implied. One is forced to wonder, if all that Fridliand said of Marat is true, why his role in 1793 did not correspond to the importance he had had as a leader of the plebeian opposition earlier. He was the champion of the lower classes and the defender of their interests, but there was a larger admixture of emotional radicalism in his make-up than this work suggests. For all that, it is an absorbing book, full of vivid detail and stimulating generalization.

University of California, Los Angeles HANS ROGGER

LA POLICE SECRÈTE DU PREMIER EMPIRE: BULLETINS QUOTIDIENS ADRESSÉS PAR FOUCHÉ À L'EMPEREUR. New Series, 1808–1809. By *Ernest d'Hauterive*. Published by *Jean Grassion*. Preface by *Marcel Reinhard*. (Paris: Librairie Historique R. Clavreuil. 1963. Pp. xvii, 777. 44 fr.) This fourth volume continues to March 31, 1809, a massive documentation (for 1804–1810) begun by the late Ernest d'Hauterive in 1922. Another volume will complete the series through May 10, 1810; Fouché's reports, annotated for Napoleon alone, are presented chronologically: complete, or in extracts, analyses, or résumés. Most are from the Archives Nationales AF^{IV} series; indicated gaps are filled from minutes in the F^7 category. One can go easily to the complete reports in the archives. A 178-page index covers subjects, personal names (over 8,000), and place names. Fouché's reports summarize an enormous flow of information from within France and abroad. Few aspects of French life or European events escape notice. One reads of Josephine ("always in tears") and the Continental System, of Talleyrand and Spain, of the daily routine (and diet) of Pius VII as political tensions enmesh him. The Church, administration, conscription, economic crisis, conspiracies, and public opinion on issues great and small—all mingle here. Fouché himself subtly presides, deftly editorializing, parrying rumors against himself, inserting suggestive marginal comments. The volume offers not major revelations but countless pieces for the larger mosaic. As its editor asserts, it is part of "the veritable secret diary of the Empire. . . ."

Washington State University HOWARD C. PAYNE

DREYFUS, DIPLOMATISTS AND THE DUAL ALLIANCE: GABRIEL HANOTAUX AT THE QUAI D'ORSAY (1894–1898). By *Thomas M. Iiams, Jr.* [Études d'histoire économique, politique et sociale, Number 42.] (Geneva: Librairie E. Droz. 1962. Pp. 157.) Iiams centers his study of French diplomatic history around Gabriel

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Hanotaux and his career as Foreign Minister. After a brief biographical sketch, he surveys the problems handled by Hanotaux, beginning with his unsuccessful struggle to obtain a protectorate over Madagascar. Iiams regards Hanotaux's settlement of the Anglo-French quarrel over Nigeria as the climax of his diplomatic career. He contends that if Hanotaux had succeeded in extracting an agreement from the British on the extent of their Egyptian claims, France would not have suffered the Fashoda crisis. In his efforts to obtain benefits from China for France, Hanotaux was ruthless and successful. His feeble efforts at Franco-German rapprochement accomplished nothing. When the Dreyfus affair burst on France, Hanotaux had some qualms at first, but soon adopted an attitude of neutrality; he even refused to disclose important information on the grounds of "reasons of state." Hanotaux negotiated a treaty with Italy over Tunisia that laid the foundation for the Franco-Italian accord of 1902. He sought to preserve the *status quo* in the Ottoman Empire lest foreign intervention lead to a European war, and for that reason he labored to maintain the concert of powers in Constantinople. Iiams has written a short, useful account of an important period in French diplomatic history. Because this is not a biography but a study in French history, Hanotaux is sometimes obscured by the details of diplomacy. As a study of French diplomacy, there are no startling revelations. Iiams has not, however, proven his conclusion that Hanotaux "must be ranked as the Third Republic's ablest foreign minister."

North Texas State University KEITH EUBANK

LE "PESO POLÍTICO DE TODO EL MUNDO" D'ANTHONY SHERLEY: OU UN AVENTURIER ANGLAIS AU SERVICE DE L'ESPAGNE. By *Xavier-A. Flores.* [Bibliothèque générale de l'École Pratique des Hautes Études, VI^e^ Section.] (Paris: S.E.V.P.E.N. 1963. Pp. 198.) The time is 1622. The Spanish government, made up of well-informed men, receives a constant stream of detailed, comprehensive data about local affairs and conditions in all parts of the world from countless diplomats, spies, friends, officials, miscellaneous servants, and the authorities of other states, as well as from various experts in Spain, and probably knows as much about the state of the world as any such group anywhere. To this is added a superfluous but steady supply of unsolicited information and advice from amateurs, foreign and domestic, thinking to solve Spain's problems or seeking to ingratiate themselves at court. Among these is the most ambitious of several submitted by Anthony Sherley, a far-traveled adventurer whose story reads like a picaresque novel. Like many, his opus deals individually and summarily with the major states, areas, and places of the world. Like most, it is a mixture of common knowledge, unreliable detail, doubtful analyses, and unlikely solutions. It is addressed to Olivares, who may or may not have read it. The time shifts to 1963. Xavier-A. Flores has located the original and two copies at the *Biblioteca Nacional de Madrid* and another copy at the British Museum, and with the encouragement of Fernand Braudel ("mon maître . . . qui m'a confié ce travail") has carefully prepared a definitive edition from the four separate manuscripts, a fifteen-page summary of Denison Ross's 1933 biography, and a short argument on the manuscript's importance. He concedes that if Olivares did read it he "n'ignorait sans doute pas la plupart des détails de cette longue leçon d'histoire que, dans un style parfois maladroit, Sherley prétendait lui faire." The same is true of any reader who recognizes Olivares' name. Flores publishes it anyway. No one knows why.

Tulane University CHARLES H. CARTER

THE DUTCH UNDER GERMAN OCCUPATION, 1940–1945. By *Werner Warmbrunn.* (Stanford, Calif.: Stanford University Press. 1963. Pp. xiii, 338. $7.50.) Werner Warmbrunn has pictured life in the Netherlands under German occupation in a

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responsible and realistic way. For that reason his historical study did not become only an epic of the Dutch resistance, but also the less glamorous story of unwilling adjustment. In this detailed and well-documented study the author describes the German governmental administration in Holland during the war and Dutch reaction to it. The purpose of the German administration was to keep the Netherlands as quiet and undisturbed as possible in order better to exploit Dutch resources for German warfare. As Warmbrunn sees it, the Germans might have been rather successful in their policy, but spoiled their chances by efforts to impose Nazi concepts on the Dutch and especially by persecution of the Jews. This persecution caused a violent reaction against the occupation regime and sparked the resistance, which grew much stronger toward the end of the war. The resistance was strongest among students, in the churches, and in the medical profession. By far the majority of the Dutch people, however, were not involved in military or even militant resistance. For most people normal life changed only gradually during the occupation. Nazi propaganda hardly had any impact on them, but traditional respect for bureaucratic authority eroded to a certain degree the spirit of uncompromising resistance. Warmbrunn makes it clear that his sympathy is with conscious resistance, not with foot-dragging adjustment. But when he describes the latter attitude, he is enough of a historian and psychologist to explain this less heroic but certainly human reaction in the depressing circumstances of the occupation.

Washington, D. C. P. G. J. Korteweg

MONARCHIA MIXTA: MAKTFÖRDELNINGSDEBATT I SVERIGE UNDER DEN TIDIGARE STORMAKTSTIDEN. By *Nils Runeby*. [Studia Historica Upsaliensia, Number 6.] (Stockholm: Svenska Bokförlaget. 1962. Pp. 619.) "The most wise men . . . affirm a mixed state to be of all others the best and most convenient to conserve the whole out of tyranny." When Pole wrote these words about 1538, he reflected an old Western political tradition carefully formulated in classical antiquity and supported by medieval political theorists. New theories of absolutism, which developed especially during the sixteenth and seventeenth centuries, stimulated political controversy on these matters and led to debates on the separation of powers carried on by a great variety of European writers. The present doctoral dissertation from Uppsala is a close examination of the course of this debate, and of the resulting practical applications of the theories proposed, in Sweden from 1611 to 1660. By the careful use of a surprisingly extensive seventeenth-century political literature, including theoretic treatises, pamphlets, academic dissertations, official documents, and various kinds of narrative sources both printed and unpublished, Runeby has done historians interested in the constitutional development of Sweden a major service. In Sweden the problem was the division of power between the king, the national functionaries, the Council of the Realm, and Parliament, and the participants in the debate freely borrowed ideas from classical writers, from Bodin and Lipsius, and from such Germans as Althusius, Arnisaeus, Besold, and Keckermann. At times, it sounds very much like political science or an essay in the history of ideas, but Runeby's consistent efforts to demonstrate the interrelationships of theory and practice break new ground and make the book a significant contribution to the historiography of the period. Although he disclaims any goal of analyzing all Swedish political literature from the advent of Gustavus Adolphus to the death of Charles X, he comes closer to this than anyone before him. In this lie both the greatest virtue and failing of the book: its incredible richness of detail, which occasionally obscures rather than carries forward the argument. Runeby seems to sense this himself when he adds summaries to three of his major divisions (1611–1632, 1632–1654, 1654–1660) as well as a review at the end. Even the reader of Swedish is grateful for the twenty-nine-page summary in German at the end.

University of California, Riverside Ernst Ekman

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HISTORIAN IN AN AGE OF CRISIS: THE LIFE AND WORK OF JOHANNES AVENTINUS, 1477–1534. By *Gerald Strauss.* (Cambridge, Mass.: Harvard University Press. 1963. Pp. ix, 296. $6.95.) This book offers a relaxed but vivid portrait of a figure who belonged to the second generation of German humanism. Dying in 1508, Conrad Celtis, the most distinguished member of the first generation, managed to escape the Protestant Revolt, the advance of the Turks to Vienna, and the sack of Rome, but Johann Turmair, his student, known as Aventinus for Abensberg, where he was born in 1477, was less fortunate. Aventinus lived till 1534, growing more despondent year by year; he is, in fact, interesting in large part for the manner in which he translated his gloom into a comprehensive vision of the past. After a varied educational experience that included Paris as well as the universities of Central Europe, Aventinus was appointed historiographer to the duke of Bavaria. In this capacity he composed his massive *Bavarian History,* which he also enlarged in a vernacular version for a wider audience; in Strauss's empathetic reading this emerges as a highly personal commentary on the destiny of man since creation. As a historian Aventinus curiously blended medieval universalism with the new history of the Italian Renaissance, and didacticism with a sense of human impotence; as a personality he was a paradoxical combination, typical of the age, of Erasmian reformist zeal with generalized pessimism. Yet this book is centered rather on the man than on the age of crisis, and although Strauss presents a memorable and moving personality, he makes less of Aventinus as a typical figure than he seems to promise in his preface.

University of California, Berkeley WILLIAM J. BOUWSMA

MILITÄRSYSTEM UND SOZIALLEBEN IM ALTEN PREUSSEN, 1713–1807: DIE ANFÄNGE DER SOZIALEN MILITARISIERUNG DER PREUSSISCH-DEUTSCHEN GESELLSCHAFT. By *Otto Büsch.* With an introduction by *Hans Herzfeld.* [Veröffentlichungen der Berliner Historischen Kommission beim Friedrich-Meinecke-Institut der Freien Universität Berlin, Number 7.] (Berlin: Walter de Gruyter & Co. 1962. Pp. xiv, 203.) In the early history of Brandenburg-Prussia, the army was an alien body in society, an instrument in the hands of the ruler that was feared and detested by most of his subjects. The reforms of Frederick William I, and particularly the canton regulation of 1733, corrected this situation by basing the army's social structure squarely upon that of agrarian society, and thus, simultaneously, nationalizing the military establishment and militarizing the peasantry and the aristocracy. This has, of course, been generally known and often stated, but less has been written about the actual working of the new military system that emerged from this social marriage. It is the great merit of Büsch's book that it tells us what it actually meant to be a cantonist, that "unhappy middle thing between soldier and peasant," and analyzes in detail the way in which every aspect of the peasant's life was dominated by his relationship to the army: his daily labors, his economic well-being, his social and political prospects. He points out that the peasant won a new kind of status—a limited emancipation—as a result of the regulation of 1733, and that his tenure of land was protected because the army depended on his production. At the same time, however, he was tied so firmly to the existing order that any hope of economic or social advancement was negligible until military defeat brought the old system to an end. The second half of this study deals with the integration of the nobility into the military system, their duties and perquisites, and the social consequences of their militarization. This section includes a careful investigation of the relationship between military discipline and agrarian justice, the economics of military administration (*Kompaniewirtschaft*), the agricultural methods of the *Junkers,* and the tendency of the military-agrarian system to rigidify with the passing of time. This study was originally inspired by Hans Rosenberg of the University of California,

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and its publication by the Berlin Historical Commission is designed in part as a tribute to him for his work in helping revive historical studies in the first years of the Free University. Rosenberg has every reason to be complimented, for Büsch's study is beautifully meticulous in its scholarship and cogent and provocative in its argument.

Stanford University GORDON A. CRAIG

JUNKER UND BOURGEOISE VOR 1848 IN DER PREUSSISCHEN EISENBAHN-GESCHICHTE. By *Dietrich Eichholtz.* [Deutsche Akademie der Wissenschaften zu Berlin. Schriften des Instituts für Geschichte. First Series, Allgemeine und deutsche Geschichte, Number 2.] (Berlin: Akademie-Verlag. 1962. Pp. viii, 243. DM 29.50.) This book provides an interesting insight not only into Prussian railroad construction during the decade preceding the Revolution of 1848, but, perhaps even more important, into the methodological differences separating historians on opposite sides of the iron curtain. The non-Marxian reader accustomed to a scholarly tradition of understatement may be irritated by obiter dicta about "plans for an atomic civil war," or by diatribes against "bourgeois-idealistic researchers" and "neo-Prussian NATO historians." Yet it would be a pity if his irritation caused him to overlook the genuine merits of this work. For the author has used important materials from several East German archives, particularly the former *Preussisches Geheimes Staatsarchiv* now in Merseburg, and, what is more, he has used them imaginatively and intelligently. To be sure, the class struggle is dragged in by the heels at every occasion, good or bad, and the reader is at frequent intervals treated to an exposition of the historical dialectic. But what does it matter? It is probably best to consider these digressions as a sort of stylistic convention, like the invocation of the Trinity in the treaties of old-style diplomacy. Having dismissed them, we can proceed to examine the data that the author has culled from Prussian government records, and even agree with most of his conclusions. There seems to be no reason, for example, to reject his contention that the state was suspicious of railroads, seeing in them instrumentalities of middle-class influence. Official reports from all levels of the bureaucracy reflect an attitude of disapproval toward a dangerous experiment, mixed with the reluctant recognition that it simply could not be prohibited. The regime tolerated railroad entrepreneurs, but it made life difficult for them, imposing restrictions and regulations at every turn. The latter were therefore forced willy-nilly to try to thwart the intentions of the government. They agreed to pay extortionate prices for *Junker* land needed for the right of way; they persuaded leading aristocrats including even members of the royal family to become shareholders in promising rail undertakings; they sought to ingratiate themselves with important figures in the civil service; and often they simply violated the law. The story that Eichholtz has told is new and significant, and while many might wish that it had been told in a less tendentious manner, they ought at least to be content that it has been told at all.

University of Wisconsin THEODORE S. HAMEROW

STRESEMANN AND THE GREATER GERMANY, 1914–1918. By *Marvin L. Edwards.* (New York: Bookman Associates. 1963. Pp. 245. $5.00.) Perhaps no other German has been subjected to more revisionist study since World War II than Gustav Stresemann. The resulting "transformation of the Stresemann image," as Ludwig Zimmermann has called it, has been considerable. Strictly speaking, the present study is not revisionist. From the first it was known that during the First World War Stresemann was intensely nationalist and annexationist. It was assumed that his character had undergone a metamorphosis in the postwar years and that he had become a "European" as well as a German. Now it seems clear that no such fundamental change took place; Stresemann adjusted to altered circumstances, but remained strongly nationalist.

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Edwards' contribution is that he etches more clearly the full extent of Stresemann's wartime annexationism and his identification with the military leadership of the state. His work was largely anticipated by Annelise Thimme in her article on Stresemann in the *Historische Zeitschrift* in 1956, although Edwards' use of Stresemann's *Nachlass* is more extensive. The adherence to an extremely narrow biographical approach reduces the value of the work for other than the specialized scholar. Although the author professes to seek a "balanced picture" of his subject, the text seems to underscore an unrelieved and intemperate nationalism to which even his willingness to work for domestic political changes was subordinated. It is in setting forth the little-known background of Stresemann's advocacy of internal reform that Edwards adds modestly to the revisionist effort.

Florida State University EARL R. BECK

DOCUMENTS ON GERMAN FOREIGN POLICY, 1918–1945. Series C (1933–1937). THE THIRD REICH: FIRST PHASE. Volume IV, APRIL 1, 1935–MARCH 4, 1936. [Department of State Publication 7439.] (Washington, D. C.: Government Printing Office. 1962. Pp. lxxvii, 1272. $4.75.) This volume of the Allied series on German foreign policy in the Third *Reich* was edited by the British team. It covers a period commencing two weeks after Hitler's denunciation of the military, naval, and air clauses of the Treaty of Versailles and ending with Hitler's plans to utilize the ratification of the Franco-Russian pact as an occasion for remilitarizing the Rhineland. The geographical focus is Europe, and the emphasis is upon over-all political policy, with military, economic, and related factors touched upon primarily where they play a direct role in political negotiations. Most of the documents deal with German relations with the Great Powers and with Germany's immediate neighbors. The Little Entente, the Baltic States, and the Balkans receive much less attention. The main theme of most of the documents is Germany's attempt to achieve by negotiation recognition as a great power on an equal basis with the former victors, to improve its European position, and to prevent the development of a clear alliance of the former Entente powers against Germany. In pursuit of this aim, the Germans cultivated Britain and Poland, maintained a cool friendship with Italy, and watched Russia and France with suspicion. A strong minor theme is Germany's policy of championing German elements outside its boundaries. Despite strong disavowals of any interest in *Anschluss,* the documents underline the German assumption that Austria would return to the German fold and confirm the general impression among historians that Austria was the chief obstacle to an understanding between Germany and Italy. In the west, Hitler refused to renounce Eupen-et-Malmédy, although prepared to abandon Alsace-Lorraine. The most important special problems handled in whole or in part in this volume are the Austrian question, the Anglo-German naval agreement of June 18, 1935, the problem of the demilitarized Rhineland, Abyssinia, and German trade difficulties. Of these questions, only the Anglo-German naval agreement is completely covered here; the other problems are continuing ones which begin earlier and are settled, if at all, later. In general, this volume provides an impression that friend and foe alike sought to placate the new Germany, even if they opposed its policy and feared its explosive energy. When Koch, the German minister in Czechoslovakia remarked, "it was obvious that, quite contrary to his attitude in the years before the Third Reich, Beneš was anxiously trying not to voice one word of criticism against Germany and to represent his rôle in international negotiations as modestly as possible . . . ," he was noting one instance of a phenomenon that applied to the Great Powers as well as the small. Where the Germans are concerned, it is clear that they enjoyed being "bottom rail on top." On the other hand, although they expected to carve out a better place for Germany as a result of this new diplomatic climate, all

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of the diplomats here represented, including Foreign Minister von Neurath, seemed sincerely to expect to do so in peace.

University of Massachusetts HAROLD J. GORDON, JR.

DIE ÖKONOMISCHEN HINTERGRÜNDE DER FASCHISTISCHEN DEUTSCHEN INTERVENTION IN SPANIEN 1936–1939. By *Marion Einhorn.* [Deutsche Akademie der Wissenschaften zu Berlin. Schriften des Instituts für Geschichte. First Series, Allgemeine und deutsche Geschichte, Number 15.] (Berlin: Akademie-Verlag. 1962. Pp. ix, 239. DM 23.) I have to admit a feeling of frustration in evaluating this book. It is intensely annoying and at the same time extremely worthwhile. Einhorn's work is the product of long, tedious, and able research in primary documents. He has used the huge reservoir of German documents of the prewar period and supplemented them with printed sources. He has not, and here is a definite weakness, used Spanish documents and sources. The information unearthed is admirable and full of important details. What makes the whole book so aggravating is the inflexible schoolbookish Marxism that makes the author victim of a laughable dogmatic bias. Dedicated to the theory that big business creates war for its own benefits, Einhorn has thoroughly investigated the German economic interaction (penetration, if you wish) of Spain, concentrating from the beginning of the Spanish Civil War to mid-1939. He believes, after unearthing many economic transactions by German capitalists, that economic reasons were crucial for German intervention. In his introduction Einhorn rudely attacks two West German scholars, Hugo Kehrer and Joachim van der Berg, for ignoring economics in the study of German-Spanish relations from 1936 to 1939. Einhorn has exaggerated. He found German economic penetration gradually increasing from pre-World War I days. But who would not? Hitler primarily supported Franco for political, ideological, and military reasons; economic factors were secondary. Indeed Einhorn has to admit that reasons other than economic imperialism also played a part in German intervention. He makes much noise, as a Marxian façade, to cover solid research and comes to a most scholarly conclusion, not much different from those of his two West German colleagues. This is a fascinating book, but Einhorn is not an exciting writer.

University of South Florida CHARLES W. ARNADE

BERLINER AUFZEICHNUNGEN AUS DEN JAHREN 1942–1945. By *Ursula von Kardorff.* (Munich: Biederstein-Verlag. 1962. Pp. 324. DM 12.80.) Students of German history need no introduction to the Kardorff family of which Ursula is the latest to make a name for herself. Her grandfather, Wilhelm, was leader of the *Freikonservative Partei* and Bismarck's trusted lieutenant in parliamentary affairs. Her uncle, Siegfried, was a prominent member of Stresemann's *Deutsche Volkspartei* and vice-president of the *Reichstag.* Her father, Konrad, gained fame as an impressionist painter; while not actively interested in politics, he left no doubt that he despised the Nazi regime and especially its anti-Semitic outrages. This background of an outstanding noble family is important to note. Without it, Ursula von Kardorff could not easily have gained so many contacts with leaders of the resistance group among the Prussian landed aristocracy and with other active opponents of the Hitler government. She had access to prominent anti-Nazis also because she belonged to the editorial staff of the *Deutsche Allgemeine Zeitung,* one of the few German newspapers that had not been completely *gleichgeschaltet,* but had miraculously maintained a semblance of independence. During the later years of the Second World War, Ursula von Kardorff kept diaries, made confidential notes on calendars, and so forth. After the collapse of the Third *Reich,* while staying in a Swabian village, she put them into a definitive form; all the tragic happenings were then still very vivid in her memory. She insists she did not add any informa-

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tion that she might have gathered after the war. She says modestly of herself that she did not do anything extraordinary and that she wanted only to describe daily life in Berlin as she witnessed it during those years of terror and sadness. In this endeavor she succeeded to a remarkable extent; no other German author caught the atmosphere of that whole period as well as she. Her diaries will be of special interest to readers who want to know more about the perilous plight of the independent journalist under a dictatorship. On many pages she gives new insights into the thinking of the courageous but unrealistic people who organized the abortive attempt on Hitler's life in July 1944. The author knew quite a few of them well, men such as Stauffenberg, Haeften, Hassell, Leber, Hardenberg, and Schulenburg. With a few bold strokes, she sketches them so that the reader understands them. This extraordinary volume was a major publishing event in Germany where it had already aroused much comment when a serialized version appeared in *Die Welt*. The book is worthy of an early translation into English.

Trenton State College FELIX E. HIRSCH

FEDERALISM, BUREAUCRACY, AND PARTY POLITICS IN WESTERN GERMANY: THE ROLE OF THE BUNDESRAT. By *Edward L. Pinney*. (Chapel Hill: University of North Carolina Press. 1963. Pp. viii, 268. $5.00.) Few scholars have delved into the inner mechanisms of the West German upper house, which plays a more significant role in the legislative process than many observers realize. A vote of thanks is owed the author for tackling a subject that defies easy generalizations and analyses. He has been successful in his aim to study the role of the *Bundesrat* in legislative policy formulation by presenting several case histories (for example, rearmament, refugee, and codetermination legislation) to test three hypotheses: the *Bundesrat* protects *Land* interests, the *Land* civil servants participate actively in *Bundesrat* deliberations, and the *Bundesrat* represents the traditional conservatism of upper houses. On occasion, this conceptual approach seems somewhat strained, but Pinney, through painstaking analysis of *Bundesrat* proceedings and other documentation and a number of interviews with West German officials, has demonstrated a sound understanding of the subject. He concludes that the *Bundesrat* plays a paradoxical role. The greater its impact on federal legislation, the less easily could national party leaders allow the *Land* governments to protect their regional interests. Hence, strains result. Pinney is, however, cautiously optimistic about the role of the *Bundesrat* in the West German democratic framework.

University of Massachusetts GERARD BRAUNTHAL

DIE GESCHICHTE ÖSTERREICHS. Volume II. By *Hugo Hantsch*. (3d rev. ed.; Graz: Verlag Styria. 1962. Pp. 626. Sch. 218.) The appearance of the third edition of this comprehensive work, reconstructing the evolution of the Austrian state from the end of the Thirty Years' War to the establishment of the first Austrian Republic after the First World War, will gratify all students of Central Europe. When the book was first published in 1950, it scarcely attracted the attention in the United States that its high merit deserved. In Europe, more particularly in Austria, the book was promptly recognized as the authoritative work on its subject. Depth of research, dispassionate learning, fairness in the treatment of the various nationalities and social groupings, warm affection for the homeland of the eminent Viennese author, and breadth of coverage explain the sustained popularity of the book among scholars and laymen alike. The new edition contains few changes. Infrequently a fresh sentence has been inserted, tenses of verbs have been modified, and page headings have been altered. For the specialist, the most valuable novelty are revisions in the notes in the nature of a bibliography. Major recent contributions, whether in books or monographs, have been incorporated, often with appraisals, some of which will not command universal agreement;

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bibliographical items that have been supplanted by fresh discoveries have been excluded. Trifling errors in citations that have been carried over from the second edition will hardly prove disturbing. It would be a service to learning if Hantsch would turn his talents to a sequel to this work, extending through the Hitlerian nightmare at least.

University of Rochester ARTHUR J. MAY

SALZBURG LUTHERAN EXPULSION AND ITS IMPACT. By *Carl Mauelshagen.* (New York: Vantage Press. 1962. Pp. viii, 15–167. $3.95.) The rich historical background of Salzburg from primitive Christian times until the expulsion of the Lutherans under Archbishop Firmian and the *Emigrationspatent* of 1731 is treated in detail. The duplicity of the archbishop, the magnificent, heroic suffering of the waves of exiles, their wonderful reception in all parts of Europe, and even in distant Georgia of the New World are, perhaps, depicted in too-glowing colors. Though based in the main on secondary accounts and undocumented, this study presents, in an area hitherto neglected, a colorful description of an event paralleled only by the Exodus of Israel in the Old Testament.

Ellicott City, Maryland ERNEST G. SCHWIEBERT

REGISTRES DE LA COMPAGNIE DES PASTEURS DE GENÈVE AU TEMPS DE CALVIN. Volume II, 1553–1564. By *Robert-M. Kingdon.* With the collaboration of *Jean-François Bergier* and *Alain Dufour.* ACCUSATION ET PROCÈS DE MICHEL SERVET (1553). By *Jean-François Bergier.* [Travaux d'Humanisme et Renaissance, Volume LV.] (Geneva: Librairie E. Droz. 1962. Pp. xvii, 141.) This work is part of a projected three-volume edition of the manuscript registers of the Genevan Company of Pastors in the time of Calvin, here given their first complete publication. These deliberations of the Genevan pastors are an important source not only for Genevan history but also for the study of Calvinist influence abroad, especially in France during the outbreak of the Wars of Religion. The editors provide copious notes identifying persons, places, and events mentioned in the text. This is a major research source; in addition, it offers exciting and illuminating reading for any student of the Reformation.

University of Missouri CHARLES G. NAUERT, JR.

ACTA TOMICIANA. Volume XIV; Volume XV; Volume XVI, Parts 1 and 2. Edited by *Władysław Pociecha.* (Breslau: Ossolinski Institute. 1952; 1957; 1960; 1961. Pp. 954; 1012; 717; 773.) *Acta Tomiciana* consist of selected documents and correspondence relating to the reign of King Sigismund I of Poland (1506–1548). The title of this voluminous publication derives from the name of Bishop Tomicki in whose chancellery, during his service as Vice-Chancellor of King Sigismund most of the materials for this collection were assembled, some being papers received or issued by the Polish Royal Secretariat, some selected from the correspondence of the Queen and dignitaries of the court and the Church. The contents of these volumes fall into the two categories of Poland's internal and external affairs, the latter not limited to Poland's immediate neighbors. Materials in both categories relate to political, economic, religious (mainly Reformation), and cultural (mainly Polish Renaissance) matters during the first half of the sixteenth century. These volumes of *Acta Tomiciana* illuminate the diplomatic relations of Poland and Turkey during the years 1532–1534. At a time when other European countries were mounting a crusade against Turkey, Poland's interests led to the signing of a peace treaty between Sigismund and Suleiman the Magnificent. The text of this treaty is the most interesting and possibly the most significant document in Volume XV. Baltic problems, too, were of great concern to the King of Poland and his advisers, Bishops Dantiscus and Tomicki. Extensive correspondence included here presents infor-

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mation on the multitude of problems pertaining especially to the duchy of Prussia and the city of Danzig. Also included is an account of the defense by the King of Poland and his bishops of the Duke of Prussia, Albert of Hohenzollern, former grand master of the Teutonic Order, before Charles V, who had placed him under the ban of the Empire for adopting Lutheranism, secularizing the order, and in 1525 creating the duchy of Prussia as a fief of the crown of Poland. In a similar context is the account that emerges from the correspondence in Volume XIV of King Sigismund's acceding to the plea of the bishop of Warmia (Ermland), Manitius Ferber, to expel the Dutch Lutherans from Elbląg (Elbing) since they exercised an undesirable influence on the Catholic laity. The *Acta Tomiciana* contain a fascinating account of the close relations of Polish men of letters with Erasmus. Among those who reflected the influence of the Dutch savant were Bishop Tomicki, Jan Łaski, who purchased from Erasmus his private book collection, and Bishop Dantiscus. In setting forth the activities of these and other Polish men of learning and distinction, the *Acta Tomiciana* perform a valuable, indeed a unique, service in unveiling the intellectual life and scholarly activities of early sixteenth-century Poland.

Library of Congress JANINA W. HOSKINS

DAS AUSLÄNDISCHE KAPITAL IN JUGOSLAWIEN VOR DEM ZWEITEN WELTKRIEG. By *Sergije Dimitrijević*. Translated from the Serbo-Croatian by *Martin Zöller*. (Berlin: Rütten & Loening. 1963. Pp. 286. DM 29.) This book deals with what Dimitrijević calls the "penetration of international financial capital" in Yugoslav banking, insurance, manufacture, and trade in the interwar period. Detailed tabular material based on data made available to the author after the revolutionary changes that took place in his country after World War II presents a variety of information on the economic connections of the Yugoslav joint companies with the foreign capital markets and on the ratios of domestic to foreign capital in their investments. The study is centered on direct foreign investment only and does not cover state and municipal borrowing. Dimitrijević shows notably that the share of foreign capital, particularly German, Austrian, French, Czech, and Hungarian, was truly enormous in the main sectors of the economy. Some fifteen of the twenty-two private Yugoslav banks were dominated by foreign banking companies; in turn these banks held a large part of the shares of the big Yugoslav mining, manufacturing, trade, and transport companies. Direct foreign investment exceeded domestic investment 6.7 times in mining, 5.1 times in the wood industry, 4.6 times in the leather and hides industry, and 4.2 times in chemicals. The foreign shares in total investment and installed power capacity and output in manufacture reached respectively 49.5 per cent, 55.5 per cent, and 45.6 per cent. On the face of these data one would think that the author would underline at least in passing the significant role played by foreign capital in the development of Yugoslav resources. The contrary is true. Dimitrijević sees in foreign investment only "exploitation," "foreign hegemony," and "subjugation" of the domestic market by "imperialism" and by "international financial capital." Throughout this biased book what is continuously stressed is that foreign investment was not socially desirable, that it influenced "negatively" the country's growth, and that Yugoslavia might have developed better and in a much more harmonious way without capital imports. Unfortunately no worth-while arguments are adduced to prove these familiar claims of many authors of underdeveloped countries. Analytically, the book adds little to the rather abundant literature on the "penetration" of foreign capital in the interwar Yugoslav economy.

Indiana University NICOLAS SPULBER

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RUSSLAND 1852–1871: AUS DEN BERICHTEN DER BAYERISCHEN GESANDTSCHAFT IN ST. PETERSBURG. Edited by *Barbara Jelavich.* [Veröffentlichungen des Osteuropa-Institutes München, Number 19.] (Wiesbaden: Otto Harrassowitz. 1963. Pp. 156. DM 20.) This collection of reports from the Bavarian embassy in Petersburg suggests the value and limitations of Foreign Office archives for Russian domestic history. Bavarian representatives were instructed to report fully on domestic questions, and periodic dispatches recorded administrative changes, economic developments, and comments on the leading personalities in the realm. Although these reports may stimulate further interest in Foreign Office archives as a source of material on domestic questions, the reports themselves are of limited research value. They are not in context and thus lose the value of continuity, while there are no comparable materials from other archives against which they may be set. Given these limitations, *Russland 1852–1871* is an interesting book, and Barbara Jelavich has done an excellent job of editing and annotating the texts.

University of Missouri R. E. McGrew

NEAR EAST

ISLAMIC PHILOSOPHY AND THEOLOGY. By *W. Montgomery Watt.* [Islamic Surveys, Number 1.] (Edinburgh: Edinburgh University Press; distrib. by Aldine Publishing Company, Chicago. 1962. Pp. xxiii, 196. $3.95.) Watt's new book appears as the first volume of a new series, "Islamic Surveys." The aim of this and future volumes to appear is to survey individual segments of the field of Islamic studies, to show the present stage of scholarship, and to assist by means of annotated bibliographies those who want to pursue their studies further. In accordance with this general plan, the book commences with a bibliographical survey of the original sources and of the studies made by Western scholars; the subsequent chapters outline the development of dogmatic and philosophical thought from its beginnings in the first century of the Moslem era (seventh century A.D.) to the present time. Each chapter concludes with a judiciously selected and annotated bibliography, supplemented by additional bibliographical hints in the appendix. The skill and perspicuity with which the author succeeded in condensing some fourteen centuries of theological and philosophical speculation into a book of less than two hundred pages deserve unreserved admiration. The lucidity of style and treatment, the meticulousness with which facts are distinguished from hypotheses, and the attention paid to the development of the Shiah, so often neglected in the books on Islam, are some of the other merits that ought to be mentioned. The weaknesses are to be attributed to the present state of the studies in this field rather than to the author himself: Western research has always paid more attention to those Moslem thinkers whose works had influenced medieval Western thought than to those who were influential in their own realm only, men such as al-Tusi, whose dogmatic treatise became the basis of an entire corpus of commentaries and glosses, as opposed, for example, to Averroës whose influence on Moslem thought was practically nil. No doubt future progress in this field will result in a considerable shift of accents, and it will be in particular the "period of darkness," the era dealt with in Part Four of this book, where a thorough re-evaluation will prove necessary. This will be the task of the future, and such objections are not meant to lessen the value of this excellent survey in which both the student of Islam and the nonspecialist will find a reliable and authoritative guide.

Princeton University Rudolf Mach

THE ECONOMICS OF CYPRUS: A SURVEY TO 1914. By *Diamond Jenness.* (Montreal: McGill University Press. 1962. Pp. xii, 219. $7.50.) This is an interesting, compe-

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tent, erudite work of synthesis, a study of one of the best-known islands of the Mediterranean from Neolithic times to 1914. Especially good are the chapters on the economy of Cyprus during the French Lusignan regime (1192–1489), during the Venetian regime (1489–1571), under Ottoman rule (1571–1878), and during the pre-1914 phases of British administration. The author devotes many fine pages to numerous Cypriot crops, including salt, sugar, cotton, wine, carobs, silk, and grains, and shows how the wine, sugar, and salt industries prospered in Lusignan times, the cotton, salt, and wine industries under Venetian rule, the production of silk during the middle phase of Ottoman rule (1680–1750), and the cultivation of grains and the potato under the British. He supplements this with estimates of the fluctuating demography of Cyprus: 150,000 in 1000 B.C., 300,000 in classical times, 100,000 in A.D. 400, 400,000 in 1340, 150,000 in 1500, almost 200,000 in 1540, 100,000 in 1770, 180,000 in 1878, 274,000 in 1911, and 500,000 in 1950. He comments perceptively on the growth of a native Greek Orthodox merchant class under Ottoman rule and discusses the subjects of deforestation in various periods of Cypriot history, road building under British direction, the collapse of the textile and small ceramic industry during the nineteenth century, and the slow rise in wages and rapid rise in rents and prices after the Crimean War. Since the author is an anthropologist by profession, his neglect of anthropology is all the more regrettable. He does, however, whet our appetite by a reference to the practice of gift exchange between borrowers and moneylenders in sixteenth-century Famagusta. All in all, he has given us an admirable economic history, with many valuable charts.

Rutgers University TRAIAN STOIANOVICH

THE KING–CRANE COMMISSION: AN AMERICAN INQUIRY INTO THE MIDDLE EAST. By *Harry N. Howard*. (Beirut: [Khayats.] 1963. Pp. xiv, 369. $7.50.) On March 20, 1919, the efforts of the Paris Peace Conference to dispose of Syria came to a standstill as a result of the conflict between British promises to the Arabs and the claims of the French under the Sykes-Picot Agreement. President Wilson proposed the creation of an interallied commission to visit the country and find out what the Syrians wanted. Although accepting it "in principle" and enlarging its scope to include the entire Ottoman Empire, the European Allies never authorized the sending of such a commission. At the end of May, following more than two months of off-again, on-again negotiation, the American members went alone. Headed by President Henry C. King of Oberlin College and Charles R. Crane, former president of the Crane Company, the commission spent almost three months in the Middle East, much of it touring Syria (including Lebanon and Palestine) and receiving petitions from representatives of many groups. Finding widespread opposition to a French mandate and to the Zionist program in Palestine, they advised against both. These adverse appraisals apparently caused the American government to withhold its report from the public. Recommendations for Asia Minor, much less controversial, were lost in the process. Howard, who published *The Partition of Turkey* in 1931, is ideally suited to tell the commission's story. Personally acquainted with some of the participants, he has had access to all the papers of the commission and its members. He has devoted a part of many years to tracking down a mountain of additional material, much of it unpublished. Where there were conflicting views, notably over Zionism and the viability of a Syrian state, all are described fully and fairly. The final chapter assesses the report and uses it to test the value of expert commissions in peacemaking. On both counts conclusions are affirmative. The author believes the recommendations were sound and that subsequent events have proved them so. A single exception was the advocacy of a mandate for Anatolia. In this respect the commission underestimated the Turkish Nationalists, but who did not in 1919? Although I am persuaded that Howard has somewhat toned down the dis-

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agreements among members of the commission, his book, a monument to patient research, seems certain to be the definitive work on the King-Crane Commission.

Kent State University JAMES B. GIDNEY

THE KURDISH REPUBLIC OF 1946. By *William Eagleton, Jr.* [Middle Eastern Monographs, Number 5. Issued under the auspices of the Royal Institute of International Affairs.] (New York: Oxford University Press. 1963. Pp. xiv, 142. $4.80.) The ease with which up-to-date ideologies can be attached to hoary political realities is illustrated by the events in northwestern Iran in 1945–1947. The Soviet-Iranian dispute drew world attention to the Azerbaijan Autonomous Republic, but the contemporaneous Kurdish Republic received relatively little notice. Eagleton has filled the gap by reconstructing the history of the Kurdish Republic from the recollections of its survivors. His account makes it clear that the Soviets were utilizing traditional Kurdish separatism and leadership. In Persian Kurdistan, Soviet agents encouraged and instigated the movement. On the other hand, the movement of the Barzanis in Iraq was entirely independent. In fact, the Russians at the time believed Mulla Mustafa to be under British influence (and so did a good many Arab Iraqis). Soviet relations with the Kurdish Republic were not free from problems. The Soviets tended to support the Azeris, who were rivals as well as allies of the Kurds. The Kurdish Republic was proclaimed without prior Soviet knowledge or approval. Kurdish aspirations for independence conflicted even more strongly with Soviet policy after April 1946, when the Soviets changed course as a result of Western pressures and bright prospects of realizing Soviet aims on an even larger scale by the penetration of the whole of Iran. When these prospects did not materialize, the Kurdish Republic was left to its fate.

University of Illinois C. ERNEST DAWN

AFRICA

THE MEDIEVAL HISTORY OF THE COAST OF TANGANYIKA, WITH SPECIAL REFERENCE TO RECENT ARCHAEOLOGICAL DISCOVERIES. By *G. S. P. Freeman-Grenville.* (New York: Oxford University Press. 1962. Pp. 238, 6 plates. $8.00.) Freeman-Grenville has written the first detailed account of the East African coast in the Middle Ages, based on extensive research in all the available literary sources plus the information that can be found from archaeological finds and oral traditions. From his account emerges a relatively clear picture of the region, disproving the existence of the often referred to Zenj Empire and recounting the history of a number of small city-states of an oligarchic nature, all united in a common cultural unit. Among these city-states was Kilwa (on the present-day Tanganyika coast), which for a time played a major role; it is the main focus of the volume. A summary of information relating to the place of the East African coast in the ancient world precedes the study of the Middle Ages, giving a valuable analysis of our limited information explaining the coast's position on the periphery of that world. A clearer idea of events comes with the Arab expansion in the centuries after Mohammed's death. Here Freeman-Grenville concentrates on Kilwa from its origins to the coming of the Portuguese, although other city-states, such as Pate and Mombasa, are examined to explain their relationships with Kilwa. All available data is considered in a lucid account of Kilwa's history. A brave attempt is made to give a picture of the daily life of Kilwa's citizens, of necessity based mostly upon sixteenth-century Portuguese documents which, if it is not all that is desired, probably accomplishes all possible at present. Valuable inclusions in the text are a map of over sixty sites for possible excavation, most of them discovered by the author, a map of the location of coin finds, and a table of the genealogy of the rulers of Kilwa.

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As Freeman-Grenville explains, Kilwa was the last outpost of the civilization of medieval Islam on the East African coast; his account of it is a work of major importance. The author has shown great skill in solving the problems facing such an attempt in an area that has only one firsthand written account, and we can hope it will set an example to be followed by others trying to recover East Africa's ancient and medieval past.

Boston University NORMAN R. BENNETT

MACLEAN OF THE GOLD COAST: THE LIFE AND TIMES OF GEORGE MACLEAN, 1801–1847. By *G. E. Metcalfe*. [West African History Series.] (New York: Oxford University Press. 1962. Pp. xi, 344. $5.60.) It is sad that Metcalfe restricted himself to writing what is mainly a straightforward, descriptive history of a facet of the government and politics of nineteenth-century British West African settlements, especially the Gold Coast, now Ghana. If he had moved a step further to impose an analytical structure upon his materials, *Maclean of the Gold Coast* would not be just a most interesting book containing valuable information for future inquiry, but a fascinating and profound book. Even so, there are an intelligent fascination and perception about Metcalfe's study simply at the level of his keen sense for relevant data to offer in a historical account centered around the experience of a particular, though not very exciting, person. One instance, among many others, of this data is the material concerning the relations between military officers and the actions of private firms or merchants to extend commerce. This curious relationship between private and government institutions (and persons) at this period preparatory to the late nineteenth-century colonial domination of all Africa is one that Metcalfe could have given more insight into if he had approached his history from an analytical standpoint. Connected with this relationship, for instance, is the complex problem of whether the flag followed trade or vice versa; a different approach by Metcalfe to his material would have deepened our understanding of this problem and the forms it took. Yet he presents an array of evidence related to this problem. For instance, Metcalfe relates *passim* the interesting situation whereby merchants secured advantage of the British government's authority by being commandants (unpaid) of outforts of Cape Coast Castle, which was itself a quasi-governmental corporation. Such merchants, significantly enough, took the responsibilities involved in their quasi-public service rather incidentally, concerning themselves, like most European merchants in West Africa during Maclean's time, with unfettered private acquisition. As Metcalfe put it, they had a distinct "inability . . . even when in responsible positions . . . to put public before private interest. In matters of trade, it was their own personal interest, not that of the merchants as a body, which came first to mind." *Maclean of the Gold Coast* is a competent book on an important period in the political history of European colonialism in West Africa; subsequent efforts toward a more systematic treatment of this period will find it valuable.

Harvard University MARTIN KILSON

ASIA AND THE EAST

CHINA: A CRITICAL BIBLIOGRAPHY. By *Charles O. Hucker*. (Tucson: University of Arizona Press. 1962. Pp. x, 125.) JAPAN AND KOREA: A CRITICAL BIBLIOGRAPHY. By *Bernard S. Silberman*. (Tucson: University of Arizona Press. 1962. Pp. xiv, 120.) These two volumes provide much-needed and valuable up-to-date surveys of Western-language (primarily English) literature (both books and articles) on China, Japan, and Korea. The principal criteria for inclusion are, according to the compilers, authoritativeness and availability, and a wide range of subjects: general, land and peoples, history, religion and philosophy, art and literature, political patterns, society, and

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so forth, are covered for each country. The compilations were made as part of a four-year program for developing Oriental studies at the University of Arizona, and most of the works listed were acquired by the university library during that period, 1957–1961. The lists contain a great number of the best books and articles on the Far East (4,218 items listed) from which should be subtracted probably several hundred cases of duplicate entry (done deliberately to facilitate use by area and topic). The fact that all entries are carefully annotated gives the lists a special value outside the realm of more comprehensive, but unannotated, bibliographies like the *Journal of Asian Studies'* yearly bibliography. No one should cavil at a useful bibliographical addition, but to do my duty as a reviewer, I should probably mention the following as minor criticisms. The authors try in each of their categories not only to list "authoritative" items, but to put them in order of "authoritativeness." This is *reductio ad absurdum* and could produce some unnecessarily hurt feelings if anyone took it very seriously, as with Norman over Sansom (*Japan and Korea,* p. 18) even though Norman is called "now somewhat dated." The narrowness of the "availability" criterion has resulted in the exclusion of numerous standard items that are readily available in large libraries. Nevertheless, students of the Far East, from beginners to professors, will find these compilations useful.

University of Pennsylvania HILARY CONROY

AKBAR THE GREAT. Volume I, POLITICAL HISTORY, 1542–1605 A. D. By *Ashirbadi Lal Srivastava.* (Agra: Shiva Lal Agarwala and Company. 1962. Pp. xxvi, 581. Rs. 20.00.) The reign of Jalal-ud-Din Muhammad Akbar is one of the great and creative periods of Indian history. His long reign of almost half a century is evenly divided between wars of aggrandizement and pursuit of truth and beauty. A great warrior, an efficient administrator, and an exceptional man of culture, his personality towers far above the many sultans and padishahs crowding the landscape of Indo-Moslem history. Akbar was both a statesman of rare vision and a shrewd realist who strove to create a monarchy that would be acceptable to Hindus and Moslems alike and also lay the foundations of a great and composite tradition of Indian culture. He has been fortunate in his biographers, ranging from the admiring Abu-l Fazl to the cantankerous Badayuni; there is also a large body of observations on his reign from Western sources. The first critical biography of the great Emperor was written in 1917 by the late Vincent A. Smith. Since then much new material has been uncovered; the present work is based on an exhaustive study of all such old and new material. Srivastava's *Akbar the Great* is the first of a projected three-volume work on the Emperor. In its twenty chapters the work narrates in a comprehensive way the crowded events in this long reign full of wars and escapades, religious search, and aesthetic delight. The author is rightly critical of many of the judgments passed on Akbar by Smith and has done well to correct the errors of dates and sequence of events in the earlier work. If Smith was prejudiced, Srivastava has an attitude of adulation toward Akbar. For instance, he argues that Akbar's unprovoked aggression against Gondawana, his vindictive carnage of the people of Chitor, and his vendetta against the House of Rana Pratap were inspired by a desire "to create the political, administrative and cultural unity of India," which is special pleading. Akbar was an imperialist plain and simple and needed neither excuse nor apology for starting a war. His was an extremely complex personality full of paradoxes and contradictions that could combine simple political opportunism and spacious idealism with rare ease. But these were parts of his greatness, and there is no reason to gloss over some of the common traits of his age and background in his character. In the very wealth of details it presents, this book fills a need; we may look forward to the subsequent volumes of this comprehensive history of Akbar the Great.

Wake Forest College B. G. GOKHALE

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THE PORTUGUESE OFF THE SOUTH ARABIAN COAST: ḤAḌRAMI CHRONICLES. WITH YEMENI AND EUROPEAN ACCOUNTS OF DUTCH PIRATES OFF MOCHA IN THE SEVENTEENTH CENTURY. By *R. B. Serjeant.* (New York: Oxford University Press. 1963. Pp. x, 233. $8.00.) Medieval southwest Arabia has received scant attention from historians, excepting some Western accounts of the invasion of its coasts in the sixteenth and seventeenth centuries by Europeans who coveted Arab control of the trade of the Indian Ocean. Arab accounts of these times and events have recently begun to attract scholars. Serjeant's firsthand knowledge of the region and of its people and their literature has led him to the discovery of a number of significant Ḥaḍramī and Yemenī manuscripts. These he has culled for passages bearing on the Portuguese and Dutch invasions, which materials are here presented in a rather free translation. The inclusion of European accounts of the Dutch pirates gives the reader some idea of the basically different outlook of invader and invaded. The texts are copiously annotated. The author draws freely on published works, manuscripts, his field records, and communications from contemporary scholars. An introduction sketching the political situation is followed by a brief account of the historical and economic background of the region. Appendixes provide valuable information on shipping terms, currency, and fortifications. Maps, illustrations, glossary, and indexes enhance the value and usability of this work, envisaged and effectively executed as a source book for the yet to be written intelligible history of southwest Arabia.

University of Chicago NABIA ABBOTT

JAN COMPANY IN COROMANDEL, 1605–1690: A STUDY IN THE INTERRELATIONS OF EUROPEAN COMMERCE AND TRADITIONAL ECONOMIES. By *Tapan Raychaudhuri.* [Verhandelingen van het Koninklijk Instituut voor Taal-, Land-, en Volkenkunde, Number 38.] (The Hague: Martinus Nijhoff. 1962. Pp. x, 230. Glds. 18.) The Dutch East India Company, established in 1602, has had settlements not only in the Indonesian Archipelago, the former Netherlands Indies, but also in Japan, China, Malaya, India, and Persia. The factories in Coromandel, the east coast of the Indian Peninsula, have been most important, for the clothes and the other commodities purchased there played a double role: they were sent partly to Holland, partly to the Moluccas as a medium of exchange for the spices to be bought there. Clothes were the main item on the list of exports from Coromandel; other principal commodities were indigo, rice, saltpeter, diamonds, iron, steel, and slaves. As the demand for imported articles was limited and inflexible, the Dutch East India Company (the author calls it "Jan Company") had to supply adequate quantities of cash specie and bullion, the items chiefly in demand in the Coromandel area. Gold and silver had to be provided from Japan and China. Thus Coromandel has been the cornerstone of the commercial system of the company in the Far East. Raychaudhuri has described the commercial situation on the coast of Coromandel in great detail and offers much that is new. He emphasizes the influence of Western commerce on the traditional economic pattern of the country. The company promoted native textile industry by introducing a putting-out system and by establishing large-scale workshops for bleaching and dyeing. "The development of a production organisation, oriented to the market, and not at all based on production for use, was one of the outstanding facts in the new situation." Not enough capital was available, however, to change the economic structure of the region completely. The new developments touched the mere fringe of the economy, and Coromandel remained an underdeveloped country. The author has made an important contribution to colonial history. Although his bibliography is practically complete, his narrative presents mainly firsthand information from the Colonial Archives in the Public Record Office in the Hague; indigenous records are apparently

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not available. The subject has been treated objectively; Raychaudhuri is entirely free from the anti-Western bias that disfigures so many publications from former colonial countries. It is regrettable that the book ends with the year 1690, the removal of the headquarters of the company from Pulicat to Negapatam. We can only express the hope that the author will continue the story until the end, that is, to the transfer of the Dutch factories in Coromandel to the British in the beginning of the nineteenth century.

University of Amsterdam I. J. BRUGMANS

THE ADMINISTRATION OF JUSTICE UNDER THE EAST-INDIA COMPANY IN BENGAL, BIHAR AND ORISSA. By *Atul Chandra Patra.* (New York: Asia Publishing House. 1962. Pp. 233. $4.50.) In 1803 the court of directors reported to the government of Bengal that British civil courts had become so congested that cases then pending would never be heard and decided within the lifetimes of the presiding judges or even of the litigants themselves. The causes of this situation and other problems of courts, judges, pleaders, and suitors comprise the substance of this study. The various sections discuss "The Courts of Justice during the Nawab's Regime," "The Mayor's Court at Calcutta," "The Sudder Dewanny Adawlut," "The Sudder Nizamut Adawlut," "The Supreme Court at Fort William," "The Courts of Circuit," "The Provincial Courts of Appeal," "The Laws in Operation." A concluding section provides general observations on trends in the administration of justice from Plassey to the Mutiny. The historian seeking an analysis of the principles of Indian law or legal theory will be disappointed. The book's title is precise. Patra's approach is functional. He is interested in the organization, personnel, and method required to operate a complex network of courts and officials, in the way justice is dispensed, and in the adaptation of statute and case and common law to meet changing conditions. The author, a practicing advocate, has relied mainly upon manuscript records and printed reports of the High Court at Calcutta. Besides these, he secured access to an unused manuscript, "Treatise on the Government of Bengal, Bahar and Orissa, with respect to the Revenues and Administration of Justice from the time of Jaffier Khan to that of Serajeddoula," covering the background period prior to company rule. The text of the present work is documented throughout by case citations and is a sober, factual, well-written account of British legal administration, with a minimum of commentary. Although some of his generalizations will prove controversial, his conclusions are rooted in the sources and, for the most part, avoid gratuitous judgments. Thus, Patra observes: "The Court of Directors had to confess that it was better to leave the Indian people to their native tribunals than to harass their feelings and injure their property by an endless procrastination of their suits under the pretext of a more deliberate justice." He concludes: "Political expediency and humanitarian considerations, though irreconcilable themselves, were the two principal guiding factors for the Administration. Logic in the sense of consistency could not be the sole aim." But, "in like situations, like determinations emanated from the courts of law, establishing equality of treatment in law."

University of California, Santa Barbara D. MACKENZIE BROWN

NANA SAHIB AND THE RISING AT CAWNPORE. By *Pratul Chandra Gupta.* (New York: Oxford University Press. 1963. Pp. viii, 227. $4.80.) Gupta's study of Nana Sahib is a superb example of a historian's ability to uncover new information and to use it with discrimination. The sources relating to Nana are seemingly scanty, inaccurate, and contradictory. The main source, British memoirs and official papers, is understandably unfavorable to the man considered responsible for the Cawnpore massacre. But Gupta has transcended these difficulties. Carefully and dispassionately sifting the information

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about Nana, Gupta has produced a vivid and convincing narrative of this Sepoy Mutiny leader. Nana has often been considered a ferocious monster. This view is wrong. He was indeed politically inexperienced and of modest talent, but he was valiant to the end despite failure. He cannot be absolved of the Cawnpore massacre, but he seems neither to have originated the scheme nor to have controlled the result. Nana was also a victim of circumstances. This new study does him justice. Proud of his heritage, possessed of grievances against the British, Nana undertook to be worthy of the leadership thrust upon him. Gupta endeavors to show, as fully as the sources permit, all the vivid details relating to the participants, campaigns, and atrocities in the Cawnpore area, in which Nana was active. The book entirely achieves this objective and should, indeed, become something of a classic, for it is unlikely that this account will be superseded, unless new sources unexpectedly appear.

Bowdoin College GEORGE D. BEARCE

POWER RELATIONS WITHIN THE CHINESE COMMUNIST MOVEMENT, 1930–1934: A STUDY OF DOCUMENTS. By *Tso-liang Hsiao.* [University of Washington Publications on Asia, Sponsored by the Far Eastern and Russian Institute.] (Seattle: University of Washington Press. 1961. Pp. x, 404. $7.50.) This is the first step in a thoroughgoing study of Chinese Communist intraparty conflicts during the 1930's and the relation of these struggles to Moscow. The 267 documents listed and annotated in this volume consist of CCP statements, resolutions, and communications related to the intraparty debates that occurred between January 1930 and October 1934, with only a smattering of non-Chinese (mostly Comintern) materials. They are grouped into twenty-five topics and arranged chronologically. A final chapter deals briefly with Mao's efforts to recast the official view of the struggles and of his gradual but nearly complete loss of power during this period. Military affairs are generally avoided, while agrarian matters will be treated in a later volume. The materials are taken almost exclusively from two sources, both available in Taiwan: CCP documents captured by General Ch'en Ch'eng's troops in Kiangsi, circulated privately in 1935, and now in the Vice-President's personal files (the Shih Sou collection); the Bureau of Investigation collection, including materials issued by CCP central headquarters, first in Shanghai and later in Kiangsi. The Chinese text of all these documents will be published later. Interviews with Chang Kuo-t'ao in Hong Kong have served to clarify many points. The annotations are essentially Hsiao's private jottings. They provide bibliographical information and then either suggest or summarize the contents of the particular document, point out its significance, and present much historical material about issues, events, and leaders, with frequent references to other related documents or discussions. They often touch on the significance of errors he has discovered in earlier works on this topic. The specialist will be interested in these and in the author's tentative conclusions. In this preliminary volume, topical orderliness is not matched by evenness nor by continuity of narration; the more general student will look forward to the editor's finished monograph.

Fletcher School of Law and Diplomacy ALLAN B. COLE

PEASANT NATIONALISM AND COMMUNIST POWER: THE EMERGENCE OF REVOLUTIONARY CHINA, 1937–1945. By *Chalmers A. Johnson.* (Stanford, Calif.: Stanford University Press. 1962. Pp. xii, 256. $5.75.) Johnson has undertaken the interesting and ambitious task of illuminating certain aspects of the "so-called Yenan period of Chinese Communist history" and of interpreting the "phenomena uncovered by historical research in terms of a general analysis of mass nationalism." The author is interested, not so much in a description of the guerrilla warfare against the Japanese

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as in the political consequences of what he calls the political and military confrontation between the Chinese peasants and the Japanese army. The key question then arises as to the link between the wartime activities of the Communists and their postwar authority. Johnson uses the example of the rise of Communism in Yugoslavia for comparative purposes. While relying on secondary sources for Chinese and Yugoslav material, he brings to his study an impressive amount of Japanese material on Chinese Communist wartime activities, including captured Chinese documents. Johnson sees the Communist rise to power in China as a "species of nationalist movement." The Chinese Communist version of Marxist-Leninist ideology is viewed as an adjunct of Chinese nationalism—a national myth serving the newly created Chinese nation-state. Communism in China today, says Johnson, is a particularly virulent form of nationalism, and nationalism is served by a combination of social mobilization (of the peasants) and national myth (Marxism-Leninism). It is argued that Marxism-Leninism is well suited to the role of national myth because it proclaims that Communists can succeed in national reconstruction, and it purports to be scientific. Johnson has made no theoretical break-through; in fact, he raises far more questions than he answers. But he is to be admired for an honest effort to be objective in a complicated field and to use the techniques of political science in a case study of the relationship between Communism and nationalism. It is not his fault that political science has inadequate tools for a project as ambitious as this one; actually, he might have been advised to lower his sights a little rather than undertake a comparative study that calls for an exhaustive knowledge of Marxism-Leninism and of Chinese conditions. Johnson is a little too free with categories such as the masses, the Chinese people, general rural support, politicization [*sic*] of the masses in China. How do we know that the "masses of China and Yugoslavia also welcomed an alliance with the first Communist state as the natural concomitant of their acceptance of domestic Communism"? The Chinese Communists certainly used Japanese atrocity stories to produce a sort of peasant nationalism against the invader, but the behavior of these peasant nationalists when given the choice not to return to Communist China after the Korean armistice should caution anyone against too much enthusiasm for an identification of Communism and nationalism.

University of Washington GEORGE E. TAYLOR

POLITIKA VELIKIKH DERZHAV NA DAL'NEM VOSTOKE NAKANUNE VTOROI MIROVOI VOINY [The Policy of the Great Powers in the Far East on the Eve of the Second World War]. By *G. N. Sevost'ianov*. (Moscow: State Publishing House for Socio-Economic Literature. 1961. Pp. 557.) Two general traditions dominate Soviet historical writing, and the value of any particular book will depend on which it fits most closely. A stringent historical empiricism that stresses source publication and heavily annotated monographs based on materials unavailable to non-Soviet scholars has produced useful works for which the historical world owes Soviet scholarship a debt. The second tradition sees history as present politics, and though the empirical strain may be reflected in the impedimenta of scholarship, this approach produces political tracts that are primarily valuable for what they say about contemporary Soviet attitudes rather than for any information on the subject discussed. Sevost'ianov's study falls into this latter category. Sevost'ianov has chosen the period 1937–1939 to show that Japan had become an aggressive, militaristic, imperialistic state, and that the roots of Japanese aggression, as well as the inability of the Great Powers to contain it, lay in the economic crises generated by capitalism. The fascist powers, Germany, Italy, and Japan, challenged militarily the established colonial position of Britain, France, and the United States. The latter powers, far from concerning themselves with the independence of the Far East or Southeast Asia, were interested in protecting the po-

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sitions they had established; consequently the only effective national liberation groups were those uncommitted to the capitalist powers. The argument is developed at great length and in immense detail. The bibliography and bibliographical essay suggest acquaintance with a wide range of sources, including American materials, though with the exception of the Russian version of the Japanese War Crimes Trials, no Japanese sources are cited. Moreover, the newspapers listed are either Russian or English, and there are no Japanese- or Chinese-language sources mentioned at all. Almost all the items in the bibliography are easily accessible, though one section lists unpublished documents and materials from the Foreign Policy Archives of the USSR and from the Ministry of Defense. These materials are used sparingly in the text to support such points as Italian backing of Japanese military and aviation programs or anti-Japanese partisan activity in China, and it cannot in any way be said that the archival data presented is central to the book's value. Indeed, published sources provide the greatest part of the study's documentation; in that sense the book presents little that is new. Since the factual information offered is unexceptional, and the argument predictable, we must conclude that the book's primary value is for the student of recent Soviet attitudes rather than for the historian studying Great Power relations in the Far East before World War II.

University of Missouri R. E. McGrew

COMMUNIST CHINA'S ECONOMY, 1949–1962: STRUCTURAL CHANGES AND CRISIS. By *Cheng Chu-yuan.* ([South Orange, N. J.:] Seton Hall University Press. 1963. Pp. xii, 217.) This is a biased but nonetheless carefully researched and exceedingly informative analysis of changes that have taken place in the economy of mainland China since 1940. It makes clear the enormity of the obstacles encountered by Chinese leaders in their efforts to bring about the rapid modernization of their backward land. Among these obstructions, none has been more formidable than the problem of how to provide incentives and to encourage initiative within the context of the collective society thrust upon China by economic necessity as well as Communist ideology. This is particularly true in the case of agriculture, where, according to the author, years of mismanagement and inadequate investment have resulted in repeated failures which more than offset the regime's spectacular achievements in the field of industrial development. It would seem that in an underdeveloped economy like China's, agriculture is too important a source of capital to be neglected, especially when soaring population gives rise to an ever-increasing demand for food. Unfortunately, Cheng's hatred of Communism causes him to indulge occasionally in practices inconsistent with high standards of scholarship. Besides using pejorative language and dubious sources, he also distorts the economic history of China before 1949 and draws unwarranted conclusions about the future of the Communist Chinese economy.

Duke University Donald G. Gillin

AMERICAS

AMERICAN FOREIGN POLICY AND THE BLESSINGS OF LIBERTY AND OTHER ESSAYS. By *Samuel Flagg Bemis.* (New Haven, Conn.: Yale University Press. 1962. Pp. 423. $10.00.) Of the seventeen essays reprinted in this attractive volume, six appeared originally in this *Review.* Of the others, three were first published in the *Proceedings of the Massachusetts Historical Society,* two in the *Proceedings of the American Antiquarian Society,* one in a book of essays, *Wilson's Foreign Policy in Perspective,* edited by Edward H. Buehrig, and the remainder in miscellaneous journals. Original dates of publication range from 1918 to 1961. A bibliography at the end of the volume lists over 75 published articles, not counting some 136 book reviews, in addition to

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Bemis' numerous well-known books. The earliest listed publication appeared in 1913. Thus Bemis, unquestionably the *doyen* of writers on American diplomatic history, is now rounding out a full half century of notable scholarly activity. It is no surprise that a sizable majority of these essays deal with the period of which Bemis has made himself the unrivaled master: the period of George Washington and John Quincy Adams. Ten essays relate to the late eighteenth century, two to the first thirty years of the nineteenth. In one, the paths of Washington and the younger Adams cross: the central theme of the famous Farewell Address is shown to bear a close resemblance to the ideas and apprehensions that Adams, then minister at the Hague, was reporting to the State Department and in personal letters to his father, the Vice-President. Among essays that appeared elsewhere than in this *Review,* and may be assumed to be less familiar to its readers, should be mentioned a highly readable one on the efforts of Washington and other Americans to secure the release of Lafayette, first from Prussian, then from Austrian, prisons; a brief and dramatic account of an attempt to involve young Charles Adams in a duel to the embarrassment of his father, President John Quincy; a sympathetic and humorous address on "The Adams Family and Their Manuscripts," delivered before the Massachusetts Historical Society in 1961 on the occasion of the publication of the first four volumes of the *Adams Papers;* an appreciation of Woodrow Wilson as the precursor of the Good Neighbor policy for the Americas and as the prophet of the still-unrealized world-wide guarantee of peace and security. The volume begins and ends on contemporary notes. The first essay, which gives the book its title, was Bemis' presidential address before the American Historical Association in 1961, wherein he contrasted, nostalgically but realistically, the national insecurity of today with the security of yesterday. The last is a plea for a congressional joint resolution that would assert for the United States the right to intervene, unilaterally if necessary, to prevent the establishment of Communist bridgeheads in the Americas. This was proposed in 1959 before *Fidelismo* had fully revealed itself. In a brief preface Bemis states that the essays are printed exactly as they appeared except for minor corrections and certain changes in titles. One factual error of some consequence (p. 196) escaped the corrector's pen.

Williamsville, New York JULIUS W. PRATT

EXPLORING THE GREAT BASIN. By *Gloria Griffen Cline.* [The American Travel and Exploration Series, Number 39.] (Norman: University of Oklahoma Press. 1963. Pp. xviii, 254. $4.95.) Since most historians have only a vague and substantially incorrect concept of the Great Basin, it might be well to state here its geographical features. It is pear-shaped, 572 miles wide at its broadest part, 880 miles long from north to south; it embraces western Utah, Nevada, and small parts of Idaho, Oregon, and California. It is distinguished as a geographical entity by a singular fact: it is an area of internal drainage, without an outlet to the sea. Mrs. Cline has written a comprehensive history of the exploration of this vast area that lay athwart the course of empire. It is a story of the Spanish, Americans, and English groping for knowledge of the contents of an area the very nature of which they did not understand. It ends with Frémont in May 1844, encamped at Utah Lake, finally describing the true nature of the vast land of interior drainage and naming it, appropriately, the Great Basin. To have attempted such a task is in a very real sense as bold an intellectual adventure as was the physical undertaking of those daring explorers about whom Cline writes. The pitfalls into which the historian may stumble are as numerous as the physical ones which made life miserable for her subjects. To the explorer's problem of "Which way shall we go?" Cline had to ask, "How much of the history of the Colorado River and the Wasatch and Sierras should I include?"; to their question of "How long will it take

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us?" the historian had to decide where to begin her narrative. Most readers, somewhere in the book, will question the author's judgments on some of these matters, but this does not seriously detract from a book that needed to be written, represents extensive research, even into Hudson's Bay Company archives, and is so detailed that all else that is necessary to physically trace the routes of her explorers is a car and a road map. Sixteen illustrations and five maps round out the narration.

Florida State University RICHARD A. BARTLETT

THE LAW MERCHANT AND NEGOTIABLE INSTRUMENTS IN COLONIAL NEW YORK, 1664 TO 1730. By *Herbert Alan Johnson*. [William P. Lyons Master's Essay Award 1962.] (Chicago: Loyola University Press. 1963. Pp. x, 85. $3.00.) The evolution of an accepted body of rules and regulations to govern and adjudicate mercantile transactions is a field little known except to the legal specialist. Originating in Roman civil law, the law merchant was altered and augmented through centuries by empirical considerations. In England the piepoudre, staple, and merchant's courts contributed significantly to this development. Mr. Johnson's brief study is concerned with the nature and use of the law merchant and bills of exchange in colonial New York. The colony's commerce during the years 1664 to 1730, decisions of its courts and action of the assembly pertinent to the law merchant are hastily sketched and evaluated. The author concludes that New York, given the advantage of Dutch antecedents, advanced more rapidly in the mechanics of certain commercial transactions than did the mother country. This was particularly true in the assignability of debt where the New York law antedated English law by twenty years. The work contains appendixes that are pertinent to the text and helpful to the reader. Unfortunately, the author's investigations are more suggestive than complete. Important aspects of the law merchant—partnership, contracts, insurance, guaranty, lien, and bankruptcy—are inadequately treated or ignored completely. Furthermore, comment about the importance of bills of credit to the economy of the colony is too brief and misleading. Final assessment of the significance of the law merchant in colonial affairs must wait for a more thorough investigation.

Miami University RICHARD M. JELLISON

WILLIAM FITZHUGH AND HIS CHESAPEAKE WORLD, 1676–1701: THE FITZHUGH LETTERS AND OTHER DOCUMENTS. Edited with an introduction by *Richard Beale Davis*. [Virginia Historical Society Documents, Volume III.] (Chapel Hill: University of North Carolina Press for the Society. 1963. Pp. xvi, 399. $7.50.) A recent hypothesis suggests that the founders of the Virginia aristocracy came from substantial middle-class English families and arrived in Virginia in the middle years of the seventeenth century with adequate financial backing. Subsequently they prospered by combining planting and official position and by 1700 were firmly established. The letters of William Fitzhugh support this hypothesis. The son of a woolen draper, he arrived in Virginia in the early 1670's. He first prospered in law, then in business and politics. In later life Fitzhugh also served as agent for the proprietors of the Northern Neck. These letters are most useful for the insight they give into the practice of the law and into the operations of a planter-merchant in seventeenth-century Virginia. The correspondence also shows what type of man it took to survive and prosper in a frontier community. Tough and hard-working, Fitzhugh was not lacking in refinement. The scarcity of "good & ingenious" society, the sad state of the church, and the difficulty of providing adequate education for his children caused him, like others of his station, to contemplate returning to England: it was better to "be never born, than ill bred." Davis has done an admirable job of editing. His introduction is enlightening, and his foot-

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notes adequately clarify most of the questions raised in the letters. Both he and the Virginia Historical Society are to be complimented on making this volume available.

University of Pittsburgh EMORY G. EVANS

BENJAMIN FRANKLIN IN PORTRAITURE. By *Charles Coleman Sellers.* (New Haven, Conn.: Yale University Press in cooperation with the American Philosophical Society. 1962. Pp. xi, 452, 44 plates. $10.00.) Every Franklin devotee will want to own this book. Divided into three sections, it is, first, a semihistory of Franklin, which emphasizes the most significant likenesses of him; next, a descriptive catalogue documenting more than 200 Franklin portraits; and, finally, a collection of more than 150 pictures of Franklin or of the people with whom he was associated. Furthermore, the volume is a handsome companion piece to *The Papers of Benjamin Franklin,* and those who own the *Papers* will doubtless also want this volume. But what does it offer to those whose interest in Franklin is less all-consuming? It is a valuable demonstration to any budding biographer of the use that can be made of a historical portrait. With imaginative and scholarly attention such as is given here by Sellers, a portrait may contribute significantly to the characterization of the subject of a biography. It need not be a mere static frontispiece. It may take the writer, and reader, into the historical past to see how the historical figure looked to his contemporaries, to peep at such things as his receding hairline, his increasing paunch, and his wardrobe. The first several chapters are significant for Sellers' general comments on the importance of portraiture. And the entire book is of value to anyone working in eighteenth-century America, England, or France, for its wealth of information on otherwise obscure painters who have left their mark in portraits of prominent men. Since the portraits are essential to an understanding of Sellers' theses, it is unfortunate that he presented them in an inconvenient form. They are in the back, in a topical arrangement that often fails to coincide with the presentation in the text. Thus it is troublesome to locate a given picture. Occasional parenthetical references to the plate on which a picture appears would have made the book much more convenient.

Los Angeles State College DONALD O. DEWEY

GEORGE WASHINGTON AND RELIGION. By *Paul F. Boller, Jr.* (Dallas, Texas: Southern Methodist University Press. 1963. Pp. xi, 235. $4.50.) It is always difficult, even for historians, to know what the real George Washington was like. In no area is this truer than in regard to his religious beliefs and practices. Pietist and freethinker have each undertaken to "prove" our first President's position. Boller has plowed through hundreds of books, essays, orations, and sermons dealing with this topic. He has also worked assiduously in the Washington papers, both published and unpublished. The result is a series of six thoroughly documented and logically developed essays: "Washington and the Pietists," "Washington as a Churchman," "Religion and the Social Order," "Washington and Christianity," "Washington's Religious Opinions," and "Washington and Religious Liberty." Carefully exposing the fallacious argument and careless assumption characteristic of much of the writing on this topic, Boller establishes several important points: Washington was less orthodox in his religious practices than has been commonly accepted; he believed organized religion indispensable to the social order; he accepted the idea of a Supreme Being who was omnipotent, infinitely wise and benign; "a total stranger to religious prejudice," Washington's position on religious liberty was "Jeffersonian in spirit" and carefully thought through. Boller believes that Washington deserves to rank with Madison and Jefferson "for establishing the ideas of religious liberty and freedom of conscience." This is a thorough and a remarkably objective study. It deserves the serious attention of scholars.

State University College, Cortland, New York RALPH ADAMS BROWN

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MOSES BROWN: RELUCTANT REFORMER. By *Mack Thompson*. (Chapel Hill: University of North Carolina Press for the Institute of Early American History and Culture. 1962. Pp. viii, 316. $7.50.) Moses Brown, youngest of four brothers of Providence, Rhode Island, spent a long and fruitful life as merchant, politician, industrialist, philanthropist, and abolitionist. His years of activity, in one or another of these causes, stretched incredibly from the 1760's to his death in 1836. Relying heavily upon superb manuscript sources, Thompson has brought together the main strands of Brown's life in a book that complements nicely James B. Hedges' economic study, *The Browns of Providence Plantations*. The major problem for the author, and one he carefully solves, is to explain Brown's motives for several unusual twists in the course of his packed life. Conversion to Quakerism after the death of his wife in 1773 makes understandable some, but not all, of Brown's chronic withdrawals from life as an entrepreneur and promoter. Thompson skillfully demonstrates how Quaker principles shored up quirks of character and personality which eventually marked Brown as a devoted humanitarian. The result is several excellent chapters on his struggle against the slave trade and slavery, refugee work during the Revolution, and compassionate service in the midst of the yellow-fever epidemic of 1797–1800. Another problem, which Thompson does not solve as well, is that of balance and proportion; this is particularly noticeable in his treatment of politics in which Brown was more deeply involved than Thompson tells us. Political success in Rhode Island during the Ward-Hopkins controversy was closely related to bribery and corruption, and Brown's faction won more often than it lost. From the author's description, one would never get the impression that his protagonist was expert in knowing where the right amount of money would do the most good at the right time. Moreover, the number of pages assigned to Brown's role in politics and his fight against parliamentary taxation is inadequate in comparison to the number given to the relocation of Rhode Island College in Providence—certainly a less significant service, yet explained here in intricate detail. Lastly, the vital relationship between the colony's politics and the revolutionary movement is hardly hinted at. Nevertheless, Thompson has written a helpful book, giving Brown a rightfully prominent place in Rhode Island history as a public servant and generally as an early American reformer and humanitarian.

University of Wisconsin DAVID S. LOVEJOY

THE MARITIME COMMERCE OF COLONIAL PHILADELPHIA. By *Arthur L. Jensen*. (Madison: State Historical Society of Wisconsin for the Department of History, University of Wisconsin. 1963. Pp. viii, 312. $4.75.) In this doctoral dissertation Arthur L. Jensen analyzes the foreign and coastal trade of Philadelphia before 1775 and the reactions of the city's merchants to British legislation after 1763. His main manuscript sources are mercantile correspondence and customs records. These enable him to present an excellent description of the commodities, shipping, tonnages, areas, and routes of the city's trade. The discussion of some subjects would have gained in strength from the use of neglected sources. Available account books, for example, would have illuminated profits and smuggling. The nature of economic conditions before the Revolution is less reliably to be sought in the correspondence of merchants than in their accounts, and in Bezanson's or Cole's price series. The final third of the book fails to carry conviction. Jensen provides insufficient support for his thesis that popular rather than mercantile forces created the revolutionary movement in Philadelphia. Moreover, his discussion is marred by unwarranted conjecture and misstatement. Opposing evidence is not marshaled and assessed. Important contradictions occur. After demonstrating that the merchants were "primarily concerned" with constitutional rather than economic aspects of the Stamp

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Act (1765), for example, he concludes that their "major grievances" prior to the reforms of 1766 were "strictly commercial."

Michigan State University STUART BRUCHEY

THE SOUTH CAROLINA REGULATORS. By *Richard Maxwell Brown.* [Publication of the Center for the Study of the History of Liberty in America, Harvard University.] (Cambridge, Mass.: Harvard University Press. 1963. Pp. xi, 230. $5.50.) This is the most revealing study of South Carolina back-country society since the publication in 1940 of Robert L. Meriwether's *The Expansion of South Carolina, 1729–1765.* It is a rare book for it reduces frontier chaos to a pattern complex enough to be close to the truth, yet simple enough to aid understanding. The Regulation (1766–1769) was an attempt to cope with the brutalizing effect that the Cherokee War had had upon the back-country people. Outlawry reached a peak in July 1766; the Regulators arose to quell the robbers. When Governor Montagu asked the assembly to suppress the Regulators, the Regulators petitioned the assembly for back-country courts and military help. The assembly passed the Circuit Court Act of 1768 and also established two companies of rangers, the latter measure being immediately helpful. The second phase of the Regulation opened in June 1768 when the Regulators decided to force the idle into work: small planters needed a labor force. As South Carolina had no vagrancy law, a plan of regulation was drawn up which provided for floggings and work service as alternatives to laziness. When those punished (there were no lynchings) began to bring suits against the Regulators in Charleston and win their cases, the Regulators refused to let the Charleston authorities serve processes in the back country. This stalemate was only resolved by the ultimate passage of a new Circuit Court Act in 1769 (the King had disallowed the first) and by the eventual setting up of courts in 1772. The more immediate cause of the end of the Regulation was that a countermovement (the Moderators) arose in the back country to thwart the Regulators. A mutual agreement to disband was reached in the spring of 1769. This book's value lies not only in its clear history of the Regulation movement, but also in its attempt to analyze the Regulators. Brown estimates that the hard core of the movement consisted of 500 to 1,000 men, but the names of only 118 have been ascertained. He analyzes the 118 in the accepted statistical fashion. Here one might challenge the method: are 118 names sufficient for such an analysis? Brown's study reveals that 69 were Whigs, 6 were Tories, and 43 were unclassifiable on this point. Is this not small authority for the generalization that South Carolina Regulators did not become Tories? A broad outline does emerge, which seems valid on the basis of common sense, bolstered by some facts. The Regulators were small planters and leading men, in late youth or in early middle age—not the biggest men in the back country but just below the top leaders.

University of South Carolina GEORGE C. ROGERS, JR.

SOLDIERS OF THE SEA: THE UNITED STATES MARINE CORPS, 1775–1962. By *Robert Debs Heinl.* Foreword by *B. H. Liddell Hart.* (Annapolis, Md.: United States Naval Institute. 1962. Pp. xxxiii, 692. $14.00.) This is the story of a body of men about whom few Americans can think without emotion. The book is hard to put down, for the author writes military history with a combination of knowledge, insight, vividness, and warmth that no living writer on the subject but S. L. A. Marshall can match. This is the story of the Marine Corps written from the inside. The author has fought in the corps and served on its headquarters staff; he is an experienced writer of its history; and he represents the intellectual element of the contemporary corps. Colonel Heinl has dedicated these exceptional qualifications to projecting on a historical screen his vision of the corps as a vital force. The result is not objective history; the author makes no

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false claims in this regard. He tells us at once that he is "both Marine and historian." We soon find that no Moslem, no early Christian was ever more sincerely convinced of the superiority of his religion, no patriot of the superiority of his country, than Heinl is of the uniqueness and demonstrable superiority of the US Marines. The proud record of the corps lends a high degree of plausibility to this conviction, and Heinl has made good his promise to try to see that record clearly and to see it whole. This is a fighting book. The author pulls no punches in dealing with those who have failed to appreciate the corps or tried to limit its freedom of growth, whether they have been "liberals," admirals, generals, Chiefs of Staff, or Presidents. His story is as much a saga of the corps's successes in fighting off attacks at home as of its brilliant exploits as a military force. The author undertakes no reasoned analysis of these attacks. The record of the corps and its support in every case by Congress and the public are enough, he feels, to justify branding its opponents as wrong. In any case, a balanced analysis of interservice contests lies outside the scope of the book. This is "Marine Corps history . . . focused on the Corps and on its members." Within this focus, the book is rich in historical information, rich in colorful anecdotes and salty characterizations that make the marine of yesterday as alive for us as the marine of today. The "Old Corps," furnishing guard detachments for the navy afloat and ashore and "colonial infantry," has evolved into the new corps whose mission is to be "the nation's ready maritime expeditionary force." If the author is right in believing that America's proper strategy is "immutably maritime," such a force linked with the navy is indispensable; equally so is the individual marine, who, throughout this book, is the American fighting man par excellence. Heinl now sees the archfoe in America's defense organization: "leveling, standardizing, monolithic, superadministered, overcentralized, unfriendly to elite units." At the end it becomes clear that the whole book is fashioned to inspire and arm marines and their friends to keep the uniqueness of the corps alive in such an environment.

Baltimore, Maryland KENT ROBERTS GREENFIELD

REINTERPRETATION OF THE FORMATION OF THE AMERICAN CONSTITUTION. By *Robert Eldon Brown.* [The Gaspar G. Bacon Lectures on the Constitution of the United States.] (Boston: Boston University Press. 1963. Pp. 63. $3.50.) This volume consists of three lectures given at Boston University during the academic year 1961–1962, in which Brown summarizes the views on colonial society, the Revolution, and the Constitution contained in his two previous books. The primary impulse of the Revolution, he says, was to preserve a predominantly middle-class society, already substantially democratized, against the encroachments of British imperialism. The Constitution, then, far from representing an "aristocratic coup d'état," was the perfectly logical culmination of the revolutionary effort: an instrument of government that the framers felt, after much deliberation and eventually with wide popular support, would afford the maximum protection for middle-class values and middle-class interests. The ideas themselves are entirely viable; they are shared in one form or another by a great many historians and are in the mainstream of present-day historical thought. But I am not at all sure that Brown's peculiar way of promoting them is very auspicious for their continued vitality and growth. From the peevish tone of this essay, I found it hard to locate the author's priorities—how much he was interested in actually developing these ideas, and how much in demanding personal recognition as their sole qualified custodian. Those present at the lectures must have fretted somewhat at the insistent themes of "Under the old interpretation, we were led to believe . . ." [but] "recent research [my own] seems to indicate . . ." marking the rhetorical rhythm of nearly every paragraph. As for the peccadilloes of Charles Beard, Brown has already made due contributions to their exposure. But he ought to be reassured that there is now substantial agreement on that question, too, and has been for some time. Indeed, the old demon was already

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marked for doom in the graduate schools by the late 1940's (it was this very setting at Columbia, for instance, that produced Robert Thomas' study of the Virginia ratifying convention), and if one is really prepared to bury the bones and get moving, I imagine that by now the road should be reasonably safe.

Columbia University ERIC L. MCKITRICK

THE LETTERS OF JAMES KIRKE PAULDING. Edited by *Ralph M. Aderman.* (Madison: University of Wisconsin Press. 1962. Pp. xxiv, 631. $10.00.) Although little remembered today, James K. Paulding was a public figure of consequence, highly regarded for his competent service as Secretary of the Navy in the Van Buren administration and well known for novels and plays in which he pioneered in the realistic use of American historical materials and local color. His published correspondence, 416 letters dated 1799 through 1860, is notable mainly for the information it contains on political and literary affairs. The largest and most interesting groups of letters—fifty to Van Buren and about half this number to Washington Irving—are within these two areas. Paulding spent his life in the shadow of men more important than himself, a fact that seems not to have bothered him very much since his own accomplishments were genuine enough and since he did not lack recognition in his own time. As Secretary of the Navy, he supervised the South Sea Exploring Expedition to the Pacific and Antarctic under the command of Lieutenant Charles Wilkes, and he devoted himself to a general tightening of discipline even in the face of an ugly squabble with Commodore Isaac Hull when that old hero of the War of 1812, without proper sanction, took his wife and sister-in-law along on a naval cruise of the Mediterranean. As an author, he collaborated with Irving in 1807 on the *Salmagundi Papers* which launched both bright young men on their literary careers. He followed with a defense of the United States, through a series of satires and burlesques, against the attacks of British travelers and reviewers; several frontier romances and tales of the New York Dutch; magazine verses and an epic called *The Backwoodsman;* a proslavery tract. His best-remembered lines are about Peter Piper picking pickled peppers. Paulding venerated Andrew Jackson, visited him at the "Hermitage" when he and Van Buren toured the South in 1842, and addressed respectful letters to him about politics and farming which, aside from their valuable insights, are fine examples of the epistolary art in its later neoclassic manifestations. His correspondence with authors includes letters to Cooper, Bryant, and Poe, but he is most enlightening on the literary scene when he writes to the hack anthologist, Rufus W. Griswold, or the high-brow New York editor, Evert Duyckinck. Aderman's arrangement is sensible, his annotation helpful, and his commentary sound.

University of Pennsylvania HENNIG COHEN

HOOSIER ZION: THE PRESBYTERIANS IN EARLY INDIANA. By *L. C. Rudolph.* [Yale Publications in Religion; Presbyterian Historical Society Publications, Number 4.] (New Haven, Conn.: Yale University Press. 1963. Pp. 218. $5.00.) Rudolph's careful study describes the Presbyterian Church's contact with and contribution to Indianians, 1800–1850. Denominational records, religious periodicals, missionary correspondence, and sermons furnish data for his lucid reconstruction of the encounter between eastern, educated, erudite ministers and the predominantly upland-southern, rude, early settlers of Indiana. The ministers of both "Schools" are accurately portrayed as Yankee agents civilizing Hoosiers in the name of Christianity, as much as frontier missionaries carrying Presbyterian religion westward. Education, the *sine qua non* to both their goals, looms large in the book. The author cogently gives Presbyterians much credit for delivering Indiana from its initial affinity with Kentucky.

Episcopal Theological Seminary of the Southwest WILLIAM A. CLEBSCH

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FIRST WHITE WOMEN OVER THE ROCKIES: DIARIES, LETTERS, AND BIOGRAPHICAL SKETCHES OF THE SIX WOMEN OF THE OREGON MISSION WHO MADE THE OVERLAND JOURNEY IN 1836 AND 1838. Volume I, MRS. MARCUS WHITMAN, MRS. HENRY H. SPALDING, MRS. WILLIAM H. GRAY, AND MRS. ASA B. SMITH; Volume II, MRS. ELKANAH WALKER AND MRS. CUSHING EELLS. Introductions and editorial notes by *Clifford Merrill Drury.* [Northwest Historical Series, Volumes VI and VII.] (Glendale, Calif.: Arthur H. Clark Company. 1963. Pp. 280; 382. $21.00 the set, postpaid.) By the skillful merging of the diaries, letters, and biographical data of six missionary brides, the first white women over the Rockies (1836 and 1838), Drury admirably attains his purpose of presenting an epic story of great Christian devotion on the Pacific Northwest frontier. Drawing upon a vast background of more than twenty-five years of research in original sources from Maine to Hawaii, Drury has woven the many threads of the Oregon Mission story into a comprehensive and well-patterned whole. Although details of "making an unheard of journey" in riding nineteen hundred miles on sidesaddles from the Missouri frontier, over the plains and the Rocky Mountains (including attendance at a Fur Traders' rendezvous) to the Columbia River, make entertaining reading, they are not as fascinating as the accounts of the daily struggles to obtain shelter, food, and clothing in the Oregon wilderness where everything depended upon individual labor and ingenuity. Each woman's journal, diary, or letters vary in depth and richness according to the individual. Those of Mrs. Whitman, Mrs. Spalding, and Mrs. Eells, although previously published, have been placed here in proper sequence and with illuminating footnotes by the editor. Mary Walker's diary, spanning ten years, holds the spotlight. Vividly she chronicles the reactions of strong-willed and strong-minded men and women forced to live in cramped quarters with no privacy, stoically enduring isolation from home folk, danger, sickness, hardships, and fear of Indians. Discord in the mission and steadily growing hostility among the Indians, particularly the Cayuses, reached a climax in 1847, with the murder of Dr. and Mrs. Whitman and twelve others at Waiilatpu. The following year, because of the great unrest of the Indians, the mission was abandoned. Much of the documentary material used in these volumes has never before been published, hence, this work is an exceedingly important contribution to Pacific Northwest history.

Denver, Colorado AGNES WRIGHT SPRING

AMERICAN SLAVERS AND THE FEDERAL LAW, 1837–1862. By *Warren S. Howard.* (Berkeley and Los Angeles: University of California Press. 1963. Pp. xii, 336. $6.50.) Back in 1896 the militant Dr. W. E. B. DuBois published his scathing indictment of an alleged slave trade conspiracy between the federal government and the slaveholders in *The Suppression of the African Slave Trade to the United States of America.* Now Dr. Warren S. Howard, armed with far more impressive archival sources, not only disposes of the conspiracy theory, but also tends to minimize the extent of actual illegal African importations during the immediate ante bellum era. Britain's humanitarian record in capturing slavers is partly rationalized as the desire of its planters after emancipation to prevent foreign planters from profiting by cheap African labor. American nationalism, fed on the bitter controversy over impressment, handicapped British efforts by permitting (in effect) foreign slavers to use the American flag. Even Lincoln hesitated to yield any American rights on the high seas until Civil War conditions threatened to bring about the wholesale extension of the brutal traffic. In deference to public opinion, Lincoln made his concessions to the British patrols as secretly as federal processes permitted. Howard's sound evidence mitigates, but does not eliminate the responsibility of the slaveholders, especially in the Deep South, where juries refused to

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convict slave crews and judges showed obvious racial bias in allowing slavers to go free. Narrow interpretations of the law, the elusiveness of the slavers and their crew, and ineffectual American policing most of the time perpetuated the traffic. Although, as the author points out, most southerners (perhaps) opposed the revival of the African slave trade during the 1850's, it is clear from the story of that agitation as told by R. R. Russel and others that a very substantial group of extremists led by the South Carolina and Louisiana politicians were indefatigable in their efforts to import Africans even at the cost of the Union.

Western Reserve University HARVEY WISH

DEPAUW THROUGH THE YEARS. Volume I, INDIANA ASBURY UNIVERSITY, 1837–1884; DEPAUW UNIVERSITY, 1884–1919; Volume II, DEPAUW UNIVERSITY, 1919–1962. By *George B. Manhart.* (Greencastle, Ind.: DePauw University. 1962. Pp. xii, 303; x, 307–596. $7.50 the set.) OCCIDENTAL COLLEGE: THE FIRST SEVENTY-FIVE YEARS, 1887–1962. By *Andrew F. Rolle.* ([Los Angeles, Calif.: Occidental College.] 1962. Pp. 191. $5.50.) These volumes fall into the collections of narrowly conceived institutional chronologies and biographical college directories in our libraries. They will satisfy those alumni who expect to find them packed with names or sketches of administrators, faculty members, and students, with pictures of glee clubs, track teams, buildings, visiting speakers, and institutional celebrations. They disappoint me because I hope that we are turning to a wider view of educational history, one that is more historical in its concern for the place of an educational institution in society and in the life of ideas. What opportunities there were in these studies, for example, to demonstrate how the changes at Methodist DePauw reflected the growing secularism of the country, or how Occidental responded to some of the demographic and social problems of southern California and the city of Los Angeles in the twentieth century. The Board of Editors of the *AHR* reminds reviewers that criticism of a book for not being another book is seldom fair. I suggest that in this case it is eminently fair: we cannot begin to understand the role of higher education in American history until we depart from dreary recitals of names, dates, and events in our institutional histories. Examples and suggestions for the direction we might take are found in the bibliographical essay of Frederick Rudolph's *The American College and University,* and in the Spring 1961 issue of the *Harvard Educational Review.*

Johns Hopkins University WILSON SMITH

THE UNIVERSITY OF MASSACHUSETTS: A HISTORY OF ONE HUNDRED YEARS. By *Harold Whiting Cary.* (Amherst: University of Massachusetts. 1962. Pp. 247. $3.75.) Here is a short chronicle of how a New England land-grant agricultural college achieved, although but recently, the purpose and patronage of a promising state university. Cary knows that the University of Massachusetts ranks historically among the most neglected of American state institutions of higher learning, but he tells his story with a sympathetic regard for the efforts of a determined band of presidents, professors, and students to overcome the particular hazards that the state university idea has had to meet in Massachusetts. His book is valuable as a record of changing administrations, courses of study, personnel, plant, and purpose, and it leaves no doubt that the institution which has become the University of Massachusetts represents a victory over no ordinary combination of evil forces. What is missing in this account, however, is any clear and sure grasp of what, when, and where these forces of evil were. The author of this centennial history leaves us wondering whether and how much the institution's history has been shaped by such factors as these: the extent to which Harvard for over two centuries had in effect been the state university, the decision of the commonwealth

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to support engineering at MIT, the vigor of the old liberal arts colleges and their jealousy of public higher education, the hostility of the Catholic Church to public education, the peculiar weakness of political morality in Massachusetts. There is no doubt, however, that at last there *is* life in the university, and perhaps a certain era of self-confidence and success will be necessary before someone can look fully at those skeletons in the university's closet that will tell the story of frustration and failure in such a way as to illuminate the history of the idea of public higher education in the United States significantly.

Williams College FREDERICK RUDOLPH

JUDGE MARVIN AND THE FOUNDING OF THE CALIFORNIA PUBLIC SCHOOL SYSTEM. By *David Frederic Ferris.* [University of California Publications in Education, Volume XIV.] (Berkeley and Los Angeles: University of California Press. 1962. Pp. ix, 149. $3.00.) John Gage Marvin (1815–1857) had been associated with the Harvard Law School and had published his learned *Legal Bibliography* (1847) when the news of the California gold discovery reached him. He needed work, and in 1850 this classmate of Francis Parkman took himself, highly educated scholar that he was, into the West. In California he founded a town, edited newspapers, and served on the bench, but his chief contribution was setting the legal frame for the public school system. Professor Ferris frequently compares Marvin with Horace Mann, and the comparison is especially valid because each was primarily concerned with the establishment of state boards, of revenue systems, and of operative machinery without concern for any abstruse educational theory. During his short tenure of office (1851–1853), Marvin saw forty-seven common schools established, introduced district libraries, emphasized modern over classical languages, and in general earned Ferris' encomium, "the founder of the California public school system." The book is a highly revealing, clearly written, and eminently useful monograph.

University of California, Riverside ROBERT V. HINE

THE SALT OF THE EARTH: A HISTORY OF NORWEGIAN–DANISH METHODISM IN AMERICA. By *Arlow W. Andersen.* ([n.p.: Norwegian-Danish Methodist Historical Society.] 1962. Pp. 338.) The very forces of social change and individual self-assertiveness that produced a large-scale emigration from the Scandinavian countries also generated an emotional and mental attitude peculiarly receptive to religions that gave free rein to the emotions, encouraged a large measure of participation in church life, and generally seemed in tune with the ideas of freedom and democracy that the immigrants either took with them to the New World or acquired in the yeasty atmosphere of the frontier. The American Methodist Episcopal Church (after 1939 the Methodist Church) was quick to respond to the needs and opportunities of pioneer life. As early as 1840 a circuit rider met with a group of Norwegian Methodists in the Fox River settlement of Illinois. In 1880, after considerable missionary activity in the Middle West, the mother church organized a separate Norwegian-Danish Conference (first known as the Northwest Norwegian Conference). For linguistic reasons, it also set up a distinctly Swedish organization. Addressing the immigrants in their native languages, through both the spoken and written word, Norwegian-Danish Methodism survived until 1943, concentrating its work in the Middle West, on the Pacific coast, and along the Atlantic seaboard. A considerable body of its records has been preserved by the Norwegian-Danish Methodist Historical Society, and Andersen, himself a product of the group he describes, has drawn freely on them. He chose to write a history replete with the biographies of leaders, administrative decisions, and reports of actions affecting organizations and institutions. From his book, the product of meticulous research,

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emerges a story of selfless sacrifice and devotion on the part of the men who made Norwegian-Danish Methodism a potent force in many communities. The history was made even more "official" by the inclusion of a valuable appendix that gives for each of the years 1880–1943 data concerning conference sessions, officials, total memberships, property values, church schools, and the like. When one considers that conference membership never quite attained to 6,500, one wonders if it might not have been more profitable to concentrate on the leavening effects of Methodism on American Lutheranism and the contribution of the conference to the Americanization of the Scandinavian immigrant.

St. Olaf College KENNETH O. BJORK

CHINESE LABOR IN CALIFORNIA, 1850–1880: AN ECONOMIC STUDY. By *Ping Chiu*. (Madison: State Historical Society of Wisconsin for the Department of History, University of Wisconsin. 1963. Pp. xii, 180. $3.50.) We have several studies of anti-Chinese activities in the American West, but comparatively little viewing the Chinese positively. Ping Chiu, using published references, census reports, and certain archives and papers, systematically calculates the involvement of this group in the California labor force to 1880 and in passing considerably illuminates the state's economy. His more important contribution is to document the place and impact of the Celestials in the various employments. Mining and railroad construction are the pursuits that have had most attention. The tables also remind us that through the Foreign Miners Tax they were for years a major source of state revenue. As of 1880 in the work force in agriculture their percentage was 14, in common labor 20, in mining 27, in domestic service 34, in fishing 39, in brickmaking 44, in gardening 38, in shoemaking 52, in laundering 80, and in cigar making 84. Because their prevailing employment was in sweatshops and low-income jobs, Ping contends that they had little to do with the job shortages of which white labor so bitterly complained. It is too bad he could not have been at the sand lots in the discontented seventies to argue this point with rabble rouser Denis Kearney.

University of California, Los Angeles JOHN W. CAUGHEY

DEAR PRECEPTOR: THE LIFE AND TIMES OF THOMAS WENTWORTH HIGGINSON. By *Anna Mary Wells*. (Boston: Houghton Mifflin Company. 1963. Pp. ix, 363. $6.00.) Colonel Higginson once said wryly, about a horse that never won a race but came in second more often than any others, that it should have been named for him. Though his was a full life, heaped with achievements and honors, something was lacking. With the passing of time it has become easy to condescend to him. Often today he is remembered as the man who wrote too many memoirs of better writers than himself, who poured out literary journalism, who fatuously called his own life story *Cheerful Yesterdays,* and above all who did his best to spoil Emily Dickinson's poetry. Thanks to mystery writer and scholar Anna Mary Wells, we now have a fuller, fairer picture. She traces his career with understanding. He entered Harvard at thirteen and graduated, second in his class of course, in 1837. He went on to join the ministry, turned to abolition and other social causes, worked as a writer and lyceum lecturer, briefly commanded an all-Negro regiment in the Civil War, wrote voluminously after the war, and ended as a national institution. Of all his activities the most significant is deemed to be his friendship with Emily Dickinson. The emphasis on her here destroys the normal structure of a biography, though the author forewarns us through the title. So much about Emily has been seen through partisan eyes that it is illuminating to have the story told from Higginson's viewpoint. On Wells's evidence he emerges as

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Emily's most steadfast and sensible friend, who cherished her as she was and never tried to force her or her poetry into the conventional nineteenth-century mold.

University of Maryland CARL BODE

TURBULENT PARTNERSHIP: MISSOURI AND THE UNION, 1861–1865. By *William E. Parrish*. With an introduction by *Robert L. D. Davidson*. (Columbia: University of Missouri Press. 1963. Pp. xvi, 242. $5.50.) By the narrow margin of 429 votes, Stephen A. Douglas won Missouri over John Bell in 1860. After the fall of Fort Sumter, a majority of Missourians were neutralists, but the militant Unionists, headed by Frank Blair, and the prosoutherners, led by Governor Claiborne F. Jackson and Sterling Price, provoked intrastate civil war. The seizure of Camp Jackson by General Nathaniel Lyon in command of Union troops and home guards (Germans) was, in the author's mind, a "colossal blunder," although this is a matter of opinion. Missouri was kept in the Union by the sagacious policy of Governor Hamilton R. Gamble at the head of the unique provisional government that took over after the flight of Governor Jackson. From 1862, political factions sought to control Gamble or to unseat him. The Radicals (Charcoals) and conservatives warred upon each other, making it difficult for the civil and military to cooperate in the defense of Missouri against Confederate invasion, suppression of bushwhackers and border guerrillas, and for harmony between civil and military authorities. The state militia relieved Union troops for campaigns in the Midwest and the East. Missouri, however, was the graveyard of military commanders who could not please political factions and one of Abraham Lincoln's worst headaches. The Radicals led by Charles D. Drake, a political opportunist and rabid emancipationist, were the only state delegation to oppose the renomination of Lincoln at Baltimore. They won the elections in 1864, rewrote the state constitution, and adopted immediate emancipation. The conservatives (Democratic party) regained control of the state in the 1870's and rewrote the state constitution, Missouri's third one in fifteen years. This volume is well written and remarkably free of mechanical errors. It portrays the most critical period of Missouri's relations with the Federal Union. The author has relied much on primary sources. He might have found the Frank Blair papers at Princeton useful.

Miami University WILLIAM E. SMITH

A BANNER IN THE HILLS: WEST VIRGINIA'S STATEHOOD. By *George Ellis Moore*. (New York: Appleton-Century-Crofts. 1963. Pp. xii, 256. $5.00.) In a sense the statehood of West Virginia represents an interesting ambiguity of the Civil War. The federal government denounced by arms the secession of the southern states, but it gave quick aid and recognition when the western counties of Virginia seceded from their mother, Confederate state. Therefore secession, particularly when viewed in the light of the top news story of 1776, would appear to be a principle of relative justification. This study of West Virginia's formation is more factual than interpretive. Moreover, the author employs a detailed outline of many headings as the form for his story. An opening chapter spotlights the trans-Allegheny and its people. The second section recounts the long antithesis between Virginia's tidewater and transmontane. It is with civil war that the narrative begins in earnest. Large numbers of Unionists in western Virginia viewed the Old Dominion's secession as "the most outrageous usurpation in the long history of eastern tyranny." A counterrevolutionary movement promptly began; two years later, after slow development and many frustrations, West Virginia achieved statehood. On this point Moore ends his narrative. Several comments, favorable and unfavorable, can be made of this work. On the debit side, compilation heavily outweighs analysis. Chapters are topical rather than chronological. Although such organization is

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necessary, subject matter tends to jump radically in time and place. Footnotes are buried together at the end and have obviously been painfully condensed. If this volume were intended as a textbook—and one gets that impression—more illustrations than three maps should have been included. An additional chapter on West Virginia's early tribulations, spanning at least the remainder of the Civil War, would greatly have enhanced the total product. Yet the narrative is not without distinct merits. It is well researched, dexterously presented, and lucid enough for young and old. The balance of subject matter is unquestionably its greatest asset, for political, military, economic, and social aspects all receive fair coverage. This is not a definitive history of West Virginia's birth; on the other hand, it is a volume that will call attention to that singular event.

George Washington University JAMES I. ROBERTSON, JR.

THE SOUTHERN CHEYENNES. By *Donald J. Berthrong.* [The Civilization of the American Indian Series, Number 66.] (Norman: University of Oklahoma Press. 1963. Pp. xvi, 466. $5.95.) Since its publication in 1915, George B. Grinnell's *The Fighting Cheyennes* has been the standard authority on that tribe's prereservation period. Berthrong has covered much the same period, but has concentrated on those Cheyennes who ranged along the Arkansas River. Trying to isolate the Southern Cheyennes has complicated Berthrong's task as they and the Northern Cheyennes exchanged visits and occasionally hunted and waged war together. His arbitrary division stems more naturally from the nature of the materials he has employed. Army and Indian Office records, which constitute most of the new material exploited for this volume, are arranged by departments and agencies that do have geographical connotations. And it is in the use of such new materials that Berthrong makes his very real contribution. Inevitably there are conflicts between the testimony of Grinnell's informants, recounting episodes of their youth or tribal tradition, and the contemporary records left by Indian agents and army personnel. Indian memory is fallible, and whites frequently either were incapable of or uninterested in accurate reporting. Reconciling these contradictory accounts would cause any conscientious scholar many agonizing and frustrating moments. The mark of it appears in this narrative, where sometimes the author seems to be writing while glancing over his shoulder at the long shadows of Grinnell and George Bent. This volume does not revivify the life of a people as did *The Fighting Cheyennes,* but by exemplary research it does supply us with a badly needed facet of their story. We now have a clearer view of Cheyenne history and a concise account of American reaction to an Indian problem. It could have been improved by additional maps to facilitate following the intricate movements of troops and Indians.

North Texas State University WILLIAM T. HAGAN

FEDERALS ON THE FRONTIER: THE DIARY OF BENJAMIN F. McINTYRE, 1862–1864. Edited by *Nannie M. Tilley.* (Austin: University of Texas Press. 1963. Pp. xv, 429. $7.50.) Recent realization of the neglect of the trans-Mississippi area in Civil War historiography makes this diary of more than ordinary interest to the student of military history of the era. McIntyre combines an accuracy of detailed observation and a talent of narration to produce an unusually revealing day-by-day account of the maneuvers of his regiment from the days preceding the Battle of Prairie Grove in 1862 through the return of the Banks expedition from Brownsville, Texas, in 1864. Although his focus is on military affairs, he also furnishes a colorful portrait of civilians encountered by his unit and the more picturesque aspects of his geographical surroundings. Of particular note is the frankness with which he discusses individual foraging accompanied by only an occasional expression of sympathy for the rebel victims. The value of the volume is enhanced by Professor Tilley's excellent editorial work in cor-

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roborating McIntyre's reliability and supplying additional information on the diary contents.

North Texas State University — JACK B. SCROGGS

FORTY MILES A DAY ON BEANS AND HAY: THE ENLISTED SOLDIER FIGHTING THE INDIAN WARS. By *Don Rickey, Jr.* (Norman: University of Oklahoma Press. 1963. Pp. xiv, 382. $5.95.) Although Bell Irvin Wiley has interpreted the life of "Billy Yank" and of "Johnny Reb" of the Civil War period, and others have written of soldier life in the armies of the West, no one has ever investigated thoroughly the role of the enlisted man who campaigned during the Indian wars from 1865 to the 1890's. Rickey's study, well written and based upon a wide variety of sources, fills a much-needed gap. The frontier soldier, Rickey points out early in his book, was an American or an immigrant who had enlisted in the army and who became a professional fighting man. He was neither a beau sabreur nor a brutalized oppressor of noble red men. What this soldier was, how he reacted to army life, and what he did are set forth plainly in a book of fifteen chapters. Beginning with the act of enlistment, the life of the volunteer is unfolded: the introduction to army life, routine duty at western posts, recreation and discipline, field service and combat, and finally discharge, and, in some cases, re-enlistment. Both cowards and heroes receive attention. The relations between enlisted men and officers are described. The author has a talent for bolstering interpretations and generalizations with specific facts and examples. At this point his research is meticulous. Although he makes the most of his sources, he does not distort them. As a result, the common soldier of the Indian wars emerges as an individual and not as a stereotype. This is no mean accomplishment and results, in part, from the judicious use of extracts from the writings of those who served and from interviews with those who fought. Some three hundred living veterans were requested to complete questionnaires pertaining to their army experience. Although not a history of western campaigns, this volume most certainly helps to reveal both the nature and the character of those who participated in the last Indian wars of the trans-Mississippi West. As such, it is a substantial addition not only to American military history in general but also a contribution to the literature of the western frontier.

University of Minnesota — PHILIP D. JORDAN

"I WILL FIGHT NO MORE FOREVER": CHIEF JOSEPH AND THE NEZ PERCE WAR. By *Merrill D. Beal.* (Seattle: University of Washington Press. 1963. Pp. xvii, 366. $6.00.) Chief Joseph of the Nez Percé Indians surrendered to General Nelson A. Miles on October 5, 1877, ending one of the last and one of the most famous Indian wars in United States history. His eloquent surrender speech struck the hearts of Americans, and Chief Joseph became a legendary figure. "Hear me, my chiefs," he said, "I am tired; my heart is sick and sad. From where the sun now stands, I will fight no more forever." Many accounts have been written of the seventeen-hundred-mile retreat of the nontreaty Nez Percé tribe, from their home in the Wallowa Valley of northeastern Oregon, across the mountains of Idaho and Montana, through Yellowstone Park, and then north toward Canada, pursued by General O. O. Howard, harassed by volunteer troops gathered along their line of march, and finally cut off by Miles. The army reports of the campaign describe the Indians as savages to be hunted down and corralled; many subsequent accounts present Chief Joseph and the other Nez Percé leaders as great patriots and military geniuses. Beal does not fall into either of these categories. He was commissioned to write his book by an arrangement between the National Park Service and Idaho State College, and he has presented an objective study based on exhaustive use of relevant sources, from both the white and the Indian side. He describes

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in day-by-day detail the movement of the Nez Percés, the strategy on both sides, the course of the battles that occurred, the conditions of the flight, and the nature of the war councils of the Indians and the whites. He analyzes the battle statistics, the numbers of warriors engaged, and the casualties on both sides. He evaluates the outcome, indicates the mistakes made, and judiciously places praise and blame where he thinks they are due. The result of all this painstaking research and writing, however, is not a striking reinterpretation of the campaign and its consequences. The outstanding features of the book are the detail of exposition and the matter of factness with which the story is recounted. Beal emphasizes anew that Joseph was not the war leader of the Nez Percés during the spectacular campaign. The nation was loosely organized, and other war chiefs were more important than he. Joseph is described rather as the guardian of his people, the one most concerned for their general welfare as a nation, and the chief who caught the public eye because he made the surrender and continued to be the spokesman for the tribe. The book is written in a simple style and is convincing in its argument. It is dramatic because of the high drama it narrates. Our understanding of the complexities of Indian-white relations, of the sorrow that attended so many of the contacts between the races, and of the certain needlessness of the suffering that arose out of misunderstandings and fear is much enhanced by a scholarly work of this quality.

Marquette University FRANCIS PAUL PRUCHA

WEST POINT AND THE PRESIDENCY. By *Frank J. Cavaioli.* (New York: St. John's University Press. 1962. Pp. iv, 154. $2.50.) This book is a brief study of the American voter's attitude toward the military elite. In comparison with Dorothy and Julius Goebel's *Generals in the White House,* which covers all the military men who served as President from Washington to Benjamin Harrison, Cavaioli's book concentrates on Grant and Eisenhower. Their electoral success is contrasted with the failure in 1864 and 1880 of those two other West Pointers, McClellan and Hancock. It is the author's contention that only West Point graduates who are not typical military men and who had strong popular appeal were able to surmont the American prejudice against professional soldiers. Since this thesis is hardly new, the chief contribution of Cavaioli's volume is its analysis of Eisenhower's career and the reasons for his political popularity. The book, though lacking an index, is satisfactorily documented with notes and bibliography. It is a useful addition to popular military history, which demonstrates adequately its conclusion that only "soldiers of democracy, such as Grant and Eisenhower, have any real chance of being elected to the Presidency."

American University ARTHUR A. EKIRCH, JR.

KANSAS CITY AND THE RAILROADS: COMMUNITY POLICY IN THE GROWTH OF A REGIONAL METROPOLIS. By *Charles N. Glaab.* (Madison: State Historical Society of Wisconsin. 1962. Pp. x, 260. $6.50.) In a sense the title of this book is illusory for it suggests a parochial, technically oriented study of limited interest. True, it is local history. Likewise, it may appropriately be regarded as railroad history. But Glaab's book has a much wider appeal and concern than either local or railroad history. The improvement and extension of transportation facilities were basic to the growth and expansion of the nineteenth-century American society and economy. It has been all too common for historians, however, to treat these developments, whether of turnpike, canal, or railroad, in a primarily descriptive fashion without sufficient examination and analysis of the forces that produced the improvements. In *Kansas City and the Railroads,* Glaab has thoroughly and skillfully analyzed the major forces that operated in a single community, focusing especially on the role of real-estate operators and tradesmen in the formulation of community policy that achieved the desired ob-

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jective: the development of railway connections that would make the city the metropolis its promoters desired. What makes Glaab's book of more than local interest is the fact that the record of American history is replete with instances of town promoters who had similar dreams, and Glaab's account not only considers directly the experiences of several other communities, but also indirectly summarizes the whole town promotional movement. But there is this one major difference: Kansas Citians, like Coates, Lykins, Van Horn, and Reid, were infinitely more successful in achieving their dream than were most of the promoters. Not only does Glaab provide a fascinating study of the development of leadership patterns and the nature of the political process in pioneer communities; he also relates what happened in Kansas City to the mainstream of events of the day. As he does this, he makes at least three new and significant contributions to our understanding of historical developments. In the first place, there is, to my knowledge, no other account of the development and employment of the device of the "paper railroad" technique utilized by the Kansas Citians. Nor have historians of the development of regulation paid attention to the role of "dissatisfied business groups in the cities of the West to protect their interests against monopolistic, consolidated enterprises." And, finally, the careful analysis of the role of business leaders in the formulation of community policy—a phenomenon generally taken for granted, but never really dissected—is a major contribution of the book. In making this distinct and valuable contribution to the literature of western, urban, and economic development, Glaab has done his work thoroughly and well, digging into both public and private records, published and unpublished sources, and has presented his findings in a most readable style.

Northwestern University HOWARD F. BENNETT

MORRISON R. WAITE: THE TRIUMPH OF CHARACTER. By *C. Peter Magrath.* (New York: Macmillan Company. 1963. Pp. viii, 334. $10.00.) Morrison R. Waite served as Chief Justice of the United States from 1874 to 1888, the period between the Courts of Marshall and Taney and those of Holmes and Taft. He entered the Court virtually unknown and has remained almost so. Magrath has provided the first biography of this long-neglected figure. Waite came to the Court the fifth choice of President Grant and found several of the justices openly hostile to his appointment. Despite this and his relative obscurity, he became an effective leader and during his tenure on the bench helped restore to the Court much of the prestige lost during the preceding two decades. The decisions of the Waite Court were notable in two areas: civil rights and economic regulation. In the first the Court reflected the national desire for conciliation and became the "guardian of the Compromise of 1877." It limited the scope of civil rights legislation under the war amendments and turned the Negro back to the states. In the area of economic regulation the Court also sided with the states. *Munn* vs. *Illinois* (1877) and subsequent decisions sustained the power of the states to regulate business. As the justices refused to adopt a judicial policy to support the new capitalism, business leaders led a vigorous attack upon the Court. They were not to be successful until the 1890's. The principal contribution of this biography lies in its analysis of the inner workings of the Court and of the Court's response to the problems of sectional reconciliation and state regulation of irresponsible economic power. In both cases the Court refused to alter the traditional relationship between the states and the federal government. While Magrath believes that Waite has been "a vastly underrated figure in American constitutional history," he readily admits that Waite "was no John Marshall." He was, however, a man of solid legal competence who managed the Court with skill and patience. The legalistic and styleless prose of the Chief Justice accounts in part for his lack of reputation as a jurist. But Magrath pre-

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sents a convincing case for the unspectacular Waite as a conscientious and able Chief Justice during a period of transition.

University of Houston RICHARD D. YOUNGER

BEATRICE WEBB'S AMERICAN DIARY, 1898. Edited by *David A. Shannon.* (Madison: University of Wisconsin Press. 1963. Pp. xv, 181. $4.50.) Before starting their massive study of English local government, Beatrice and Sidney Webb visited briefly other English-speaking countries. Disembarking at New York on March 30, 1898, they left for Hawaii on July 7. Readers of the bulky volumes published earlier from Mrs. Webb's diaries will expect touches of prudery and upper-class British snobbishness. But they will also expect some marvels of concise precision, and they will find them here: the analysis of the changing political system of New York and its relation to the social structure, a sketch of Thomas B. Reed and the machinery by which he ran the House of Representatives, the deplorable fiscal practices of governments, how the theory of checks and balances led to inefficiency or to autocracy, American distrust of representative government and its consequences. The six pages on Carnegie Steel, with its executives who took no civic responsibility whatever, are remarkably enlightening. Although Mrs. Webb repeated the stale and very dubious allegation that the United States was producing fewer important thinkers than Western Europe, she found domestic architecture and home decoration much better here than in England. Students of the period are indebted to Shannon for getting this book into print, but the editing seems perfunctory at times. Of the Americans who merit entries in his biographical directory, more than 80 per cent (55 of 68) are listed in the *Dictionary of American Biography.* Conversely, I would have appreciated brief sketches of Emily Tuckerman, Edward Colston, and Superintendent McGrew, and I would surely like to know at least the name of the organizer of the book trust that reportedly supplied five-eighths of all schoolbooks in the country and bribed school officials to get the business.

Brandeis University RAY GINGER

THE QUAKER STAR UNDER SEVEN FLAGS, 1917–1927. By *John Forbes.* (Philadelphia: University of Pennsylvania Press. 1962. Pp. 274. $6.00.) This is a history of the first ten years of the American Friends Service Committee, the response of American Quakers to war and conscription and their quest for William James's "moral equivalent of war." The volume covers the organization of AFSC and the launching and liquidation of relief and reconstruction programs in France, Serbia, Austria, Germany, Poland, Upper Silesia, and Russia. An epilogue sums up the experience; extensive footnotes, a brief list of related readings, and a fair index conclude the work. The author is a Quaker, but there are no indications that his unobtrusive sympathy with the AFSC's purposes have affected his historical judgment. AFSC's achievements are allowed to speak modestly for themselves, and due credit is given to the roles of the American Relief Administration, the American Red Cross, and to British Friends who often led the way into new areas of need. The focus is sharply on relief and the problems of organizing and administering it encountered by AFSC. The author's prime source is the AFSC archives at Haverford College, and he develops much useful and interesting information concerning conscientious objectors (draft evaders outnumbered them sixty-three to one), the arrangements worked out with the War Department for alternative service, the problems of a pacifist group working with an agency virtually an arm of the government (ARA), the hypersensitivity of host governments. Forbes conveys something of the unique motivation of AFSC's workers and the legacy of each relief operation: a hospital in France, an agricultural school in Serbia, the *Deutscher Zentralausschuss für die*

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Auslandshilfe in Germany, a school for nurses in Russia. In this century of total wars it is good to have the record of a ministry of love.

Pennsylvania State University JOHN E. PIXTON

THE MAN WHO RODE THE TIGER: THE LIFE AND TIMES OF JUDGE SAMUEL SEABURY. By *Herbert Mitgang.* [Legal Studies of the William Nelson Cromwell Foundation.] (Philadelphia: J. B. Lippincott Company. 1963. Pp. xx, 380. $6.95.) Seabury's principal claim to fame derives from his three sensational investigations into New York City corruption in the early 1930's that led to the resignation of Mayor "Jimmy" Walker, the smashing of Tammany, and the elevation of Fiorello H. La Guardia and reform to city hall. Even before this time, however, Seabury had earned a reputation as a vigorous enemy of machine politics and as an outstanding champion of municipal reform and liberal causes. More than any other New Yorker, he embodied for his generation the city's civic and social conscience. Several books deal with the story of the Seabury investigations, but until the publication of Mitgang's biography, the only biography of Seabury was that by Walter Chambers (*Samuel Seabury: A Challenge*) which came out in 1932, even before Walker had been publicly implicated by Seabury's greatest investigation. Readable, well-proportioned, and generally well-researched, the Mitgang book is now the standard biography of Seabury. The heart of the research involved interviews with people who had known Seabury, especially with those who had served on Seabury's investigative staff, his "boys"; the reminiscences of the Seabury "boys" add flesh and color to the story of the inquiries justifying its retelling. There are occasional errors of fact; for example, the mayoral campaign Mitgang places in 1895 occurred in 1894, and it would have been difficult for Big Tim Sullivan to have "licensed" Arnold Rothstein's gambling places in the 1920's since Big Tim had died in 1913. In one or two places, a more adequate treatment is required. The author's handling of the Seabury-Berle plan for transit unification is superficial and unbalanced. Especially in light of Seabury's liberal past and early approval of the New Deal, when and why was he alienated from the Roosevelt administration? In my opinion, the book would have been strengthened had Mitgang permitted himself to be somewhat more critical of his subject at times. Historians will regret the absence of footnotes (except for the citation of court cases) and the rather skimpy bibliography.

Long Island University CHARLES GARRETT

THE TWO-OCEAN WAR: A SHORT HISTORY OF THE UNITED STATES NAVY IN THE SECOND WORLD WAR. By *Samuel Eliot Morison.* (Boston: Atlantic-Little, Brown. 1963. Pp. xxvii, 611. $15.00.) This handsome, bulky, and rather expensive *Short History* is a condensation of Admiral Morison's fifteen-volume "History of United States Naval Operations in World War II." Though its primary purpose is to bring "the exploits of the United States Navy in World War II to the attention of new readers," it is not a good sample of the best work of one of the greatest of American historians. The larger work's style and emphasis on commanders' personalities come through very well, and some of its battle narratives have been clarified by compression, but college undergraduates might better be introduced to Morison by some of his other volumes. This condensation is not as successful as the admittedly less drastic compression that resulted in the fine *Christopher Columbus, Mariner,* his *Oxford History of the United States,* or Captain S. W. Roskill's analogous condensation of the Royal Navy's official history, *White Ensign.* Morison has more than twice as many charts and diagrams and about the same number of pages of photographs as Roskill. Eleven of the twenty-five pages of photographs are devoted to naval commanders. Roskill has none at all. General of the Army Eisenhower is shown with two rear admirals. There is no pic-

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ture of another general of the army whose place in the projection of American power overseas was surely as significant. Still more important, "Operations" no longer appears in the title. Neither Morison nor Roskill mentions that other official or semiofficial volumes have dealt with such mundane phases of naval history as industrial mobilization or logistics, though Morison thinks that the United States Navy's "at-sea logistics system" was "one of the principal instruments of victory." A new introductory chapter has replaced an unsatisfactory one by another hand. The fine nine-page conclusion is too short for Morison's reflections on the place of sea power in this great war or in twentieth-century American history. Though this volume was probably not the place for such reflection, it can be hoped that Morison will follow this summary with a work that compares with Roskill's *Strategy of Sea Power* on the scale of his earlier *Strategy and Compromise*.

Duke University THEODORE ROPP

TRIUMPH IN THE PHILIPPINES. By *Robert Ross Smith*. [U. S. Army in World War II: The War in the Pacific.] (Washington, D. C.: Office of the Chief of Military History, Department of the Army. 1963. Pp. xxi, 756. $8.00.) Here is the story of the reconquest of the Philippine Islands, except for Leyte and Samar (handled in another volume), covering the first six months of 1945 and the biggest campaign of the War in the Pacific. The magnitude may be indicated by the cost in dead: to the United States, 10,380, and to Japan the unbelievable figure of 255,795, nearly 25 times higher. This grim holocaust is described in fine, clean prose. More clearly written than some of the volumes in the "U. S. Army in World War II Series," this one yet shares with them a bloodless quality. Although the term "bloody" appears from time to time, it is as close as the author comes to imparting to the reader the flesh-rending side of war. What this volume, and the series in general, offers the reader is a clinical narrative in bountiful detail of a specific segment of the science and art of war. If the book reaches a climax anywhere, it is in the reconquest of Manila, the only prolonged fight during the entire conflict to take place within a major city. As such it was of special significance. And for many of the high command, the reconquest was a wresting of the flower of the American empire away from the infidel. Though General MacArthur would not allow aerial bombardment, the heaviest cannon of the field artillery reduced the city to rubble. For the general reader, Part One, "Plans and Preparations," is the most interesting, though not the most original. For the specialist, there is value in the method of presenting the Japanese as well as the Allied situation. And finally, for everyone, there is real joy in the splendid maps, charts, pictures, appendixes, and index.

University of Florida JOHN K. MAHON

MILITARY ADVISORS IN KOREA: KMAG IN PEACE AND WAR. By *Robert K. Sawyer*. Edited by *Walter G. Hermes*. [U. S. Army Historical Series.] (Washington, D. C.: Office of the Chief of Military History, Department of the Army. 1962. Pp. x, 216. $3.50.) This official study by a professional soldier and a professional military historian recounts the experience of one of the first military advisory groups to be formed. In its efforts to help organize and train the South Korean forces, KMAG encountered many problems: language and communication barriers; an unstable economy; a dearth of trained native administrators, technicians, and military leaders; internal strife; guerrilla operations and skirmishes along the thirty-eighth parallel; and different standards of efficiency and operations. When the Korean War erupted, only limited progress had been made in improving the republic of Korea army's organization, leadership, and defenses. Yet a sound base of reform had been laid, and with the stabilization of the fighting in mid-1951, KMAG could be more deliberate in the pursuit of its ob-

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jectives. Sawyer and Hermes have offset sparse documentation by drawing on interviews and correspondence with former KMAG team members. Their account should prove of value and interest, especially in light of America's present wide-ranging and increasingly important efforts to bolster underdeveloped and newly independent peoples.

Industrial College of the Armed Forces HARRY B. YOSHPE

MUSKOKA AND HALIBURTON, 1615–1875: A COLLECTION OF DOCUMENTS. Edited with an introduction by *Florence B. Murray*. [Ontario Series, Volume VI.] ([Toronto:] University of Toronto Press; the Champlain Society for the Government of Ontario. 1963. Pp. cxiv, 445. $5.00.) This volume, the sixth of a series on Ontario local history, is concerned with the rugged "shield" region between the Ottawa River and Georgian Bay. The documents, many published for the first time, relate to Indians and missionaries, the fur trade, lumbering, and agriculture and transportation, indeed to almost every aspect of the area's history from the first explorations in the seventeenth century to the present. Miss Murray's introduction, obviously a product of exhaustive research, transcends the bounds often imposed upon themselves by local historians and describes the region in a wider setting. As a result, the volume should be of interest to students of frontier history as well as to the citizens of Ontario to whom the Prime Minister in his foreword commends it.

University of California, Los Angeles JOHN S. GALBRAITH

THE ABORIGINAL POPULATION OF CENTRAL MEXICO ON THE EVE OF THE SPANISH CONQUEST. By *Woodrow Borah* and *Sherburne F. Cook*. [Ibero-Americana, Number 45.] (Berkeley and Los Angeles: University of California Press. 1963. Pp. 157. $3.50.) Previous studies by these authors have measured the native population loss in central Mexico from 1531 to 1610, but have not directly confronted the problem of population in 1519. Reverse extrapolations from the graphs already constructed yield figures between 32,000,000 and 50,000,000, but the authors argue that all such reverse extrapolations are deceptive because population remained relatively stable for a short time following the conquest. The delay cannot be documented, but its plausibility, after the analogy of unspecified laboratory experiments with animals, prepares the reader for the unexpectedly low total reached for the preconquest period. This total is 25,200,000, and it compares with 16,800,000 in 1532, 6,300,000 in 1548, 2,650,000 in 1568, and still lower populations in subsequent years. The new figure is computed mainly from three related documents, the *Matrícula de tributos, Codex Mendoza,* and the testimonies of 1554 published by Scholes and Adams. Total annual tributes are compiled for the various provinces, an estimate is made of the average payment per tributary family (one mantle or two *fanegas* of maize or beans), a further estimate is made of the number of persons per family (4.5), and a conjectured nontributary population is added. For some areas that cannot be accommodated to these techniques, other kinds of extrapolation, from the central plateau or from the figures for 1532, are used. Obviously the statistical base is less firm than in the previous studies, the amount of supposition greater, and the whole structure weaker. There are some careless errors (the province of Hueypuchtla is listed in both Region I and Region II), but given the large number of indeterminate factors it is unlikely that more precise conclusions than these could be reached. The authors themselves are careful not to insist on the 25,200,000 figure. They allow for a range, based on the same evidence, between about 18,800,000 and 28,000,000.

State University of Iowa CHARLES GIBSON

PRINTING IN COLONIAL SPANISH AMERICA. By *Lawrence S. Thompson*. [Archon Books.] (Hamden, Conn.: Shoe String Press. 1962. Pp. 108. $3.00.) This neat

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booklet, with its forty-eight facsimile illustrations of colonial imprints, includes running notes in brief chapters on presses, printers, and their products in the various areas of the former Spanish Empire in America. Lacking a bibliography and footnote citations, it is not clear how extensive the sources used were, but chief reliance was apparently placed on the authoritative studies of the Chilean bibliographer, José T. Medina. The larger works on the same subject by the Argentine scholar, José Torre Revello, such as his *Orígenes de la imprenta en España y su desarrollo en la América española* (1940), it appears, were not consulted. Some statements made are disquieting. I am saddened by the comment, "there were no *libros de caballerías*, so beloved in Spain," since my *Romances of Chivalry in the Spanish Indies* laid low that canard thirty years ago. The *Thomasiada* (1667) of Diego Sáenz Ovecuri is described as "an epic poem of genuine merit" when it was one of the most bizarre exhibitions of verbalistic tricks and devices of the baroque age of colonial letters. Despite such shortcomings, this brief treatise is useful as an introduction to the subject for the reader unable to use Spanish.

University of Michigan IRVING A. LEONARD

MORELOS OF MEXICO: PRIEST, SOLDIER, STATESMAN. By *Wilbert H. Timmons.* (El Paso: Texas Western College Press. 1963. Pp. xiii, 184. $5.00.) This first full-length biography of Morelos in English is a welcome addition to the growing library of political and historical works on Mexico. Because of the limitations of his source material, Timmons has concentrated his major attention on Morelos' military career, but he has not entirely neglected his subject's earlier ecclesiastical life nor his interest in government and social reform. From the author's evidence, however, it seems clear that Morelos' motives for entering the priesthood were largely material (that is, to obtain an ecclesiastical benefice to which he could lay claim); his abandonment of that career and his sexual indiscretions are best explained in those terms. Concerning Morelos' third career, that of reformer, the author himself contends that Morelos' main purpose at all times was the destruction of the Spanish government in Mexico and that his economic and social proposals, best outlined in his *Medidas políticas* . . . and "Sentiments to the Nation," were but means to that end. With respect to the famous Constitution of Apatzingán, the author offers conclusive evidence that Morelos' influence on that document was negligible; in fact, Timmons believes that Morelos did not even arrive in Apatzingán until the draft of the Constitution was almost completed. In providing for a weak executive and a strong legislature the Constitution established a government directly contrary to Morelos' aspirations. However serious Morelos was or was not in presenting his programs of social and political reform, Timmons would have made an important contribution to intellectual history had he also included some speculation on the origins of Morelos' doctrines. What kinds of books did Morelos read in the schools and seminary he attended? Did he have a library in his rectory, and what books did it contain? Who among his associates seemed to have had intellectual influence over him? Perhaps these questions cannot be answered definitely, but even if they cannot, the author's surmises would still have proved valuable.

University of Texas KARL M. SCHMITT

DOCUMENTOS HISTÓRICOS DE LA REVOLUCIÓN MEXICANA. REVOLUCIÓN Y RÉGIMEN CONSTITUCIONALISTA. Volume III, CARRANZA, WILSON Y EL ABC. Edited for the Comisión de Investigaciones Históricas de la Revolución Mexicana under the direction of *Isidro Fabela.* [Fuentes y documentos de la historia de México.] (México, D. F.: Fondo de Cultura Económica. 1962. Pp. 414.) The pieces of the jigsaw puzzle comprising the many-sided struggle in Mexico from 1910 to 1920 are slowly falling into place. To aid the process, these volumes by former Secretary of

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Foreign Affairs Fabela are providing scholars with a highly specialized collection of the pieces. The editor's purpose apparently is to prove that the conduct of Venustiano Carranza, the First Chief, was uniformly correct and patriotic while those who disagreed with him were deluded by personal ambitions, or worse. Each selection is followed by a commentary, often longer than the original, to make sure that the proper interpretation is given. Original telegrams, valuable diplomatic correspondence and instructions, newspaper editorials, plain polemics, and secondary accounts are included with little apparent selectivity, except that each contributes to build up the impeccable Carranza administration. Victoriano Huerta, the dictator Woodrow Wilson was so anxious to eliminate, receives only adverse comment. Other Mexican leaders of the time are largely ignored though the part played by Pancho Villa is now at least partially revealed in the recent publication of C. C. Clendenen, *The United States and Pancho Villa*. This third volume of the series by Fabela carries the title of *Carranza, Wilson and the ABC*. In developing this theme the editor insists that the only legitimate activity of the ABC powers was to try to settle the United States-Mexican difficulties with no attendant right whatever to consider internal conditions in Mexico. Such a position certainly had legal precedents, but the negotiators were faced with a major problem: if they eliminated the dictator, Huerta, would they not simply leave a power vacuum in Mexico in which from two to a half-dozen claimants stood ready to engage in a vicious struggle for control? In the maneuvering for a solution that ensued, the First Chief managed to keep one foot in the door of the negotiations, the while, on the basis of technicalities, he refrained from becoming so involved as to have to assume any obligations. So skillfully did he play the game that he emerged with *El Gordo* (as the chief prize in the lottery is called) in the form of *de facto* recognition as President of Mexico. The very bias of this collection makes all the more evident to the scholar the baffling difficulties that faced the negotiators. Finally they reached the conclusion that, in spite of his stubbornness, Carranza was the "best bet" in the confusing Mexican merry-go-round. For the United States it had become an annoying sideshow on the periphery of World War I, but for Mexico it was still a vital issue of national integrity and of independence itself.

University of South Carolina W. H. Callcott

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* The lists of articles are compiled by the section editors whose names appear. The listed books are those received by the *Review* between April 15 and July 15, 1963.

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Historical News

Editorial

Time for Action?

American historians share in the age of affluence, most of them as poor relations. American historians, or many of them, share the pragmatic dream that reasonable solutions may be found to society's problems. They believe that knowledge of history might light dark alleys to the future and agree with Patrick Henry that "there is no way of judging the future except by the past."

Americans are a people beset by gadgets, intrigued by scientific wonders, and lulled by tranquilizers. They seldom oppose the humanities. They relegate them to a secondary role—nice to know, but of little real value.

If we look at ourselves in a mirror, we may see blemishes that explain in part why the study of history is not a primary concern. American historians, too often, are indifferent to form and to the meaning of what they write. Perhaps because they write to publish rather than to be read, they ignore Emerson's advice, "Tell us what you know"; rather they tell us what they have found and how they have found it, "a longer and a drearier tale." Perhaps because they think too little about the interest or significance of their findings, they fail to excite their compatriots and students. As historians we are rightly fearful of the dangers of presentism. But truth be recognized, some of our writing induces sleep, and some of our research and teaching is neither significant nor interesting.

History and historians do not fully share in the present affluence. The fault is partly ours, but only partly. We work in a society whose educational funds are now largely dispensed by foundations and legislatures. We may be thankful that the great foundations have aided historical work (of this Association, for example) during recent years, but they have not been, nor are they now, eager to do so. State and local governments support the study of history in the schools, universities, and historical societies; this support is often reluctant and never ample. Federal funds pour into scientific research, especially for the military, but dribble into historical studies. Foundations favor novel and "periphery" ventures, underdeveloped areas, and the sciences; history, an old humanity, seldom attracts them. Congressmen, impressed by the usefulness of science in the cold war, rarely understand how any history except the most patriotic makes a contribution to the national well-being, and they fear that history may be used to promote subversive ideas.

As scholars we cannot assert that the study of history will enable us to beat the Russians or save mankind. Nevertheless we know, with Cicero and Santayana, that "when experience [history] is not retained, as among savages, infancy is perpetual."

The study of history *is underdeveloped* in the United States. We have not, far from it, exhausted the riches of the past. Aware of our blemishes and convinced of the value of our subject we may, more confidently than we have, ask our administrations, foundations, and governments to assist us in meaningful recovery of the past. But what they will give us depends upon how affluent we are in our learning and in sharing it. We can know more and tell our fellows more of what we know —the student to enrich his specious present, the legislator to widen his choices, the judge to reveal what is humane, the statesman to show him possibilities. Have we the intelligence, the conscience, the will? If so, it is time for united action.

Boyd C. Shafer

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American Historical Association

As has been announced, the Association meeting for 1963 will be held at the Sheraton and other hotels, Philadelphia, Pennsylvania, December 28–30.

In 1964 the Association will meet at the Sheraton Park and Shoreham Hotels, Washington, D. C., December 28–30. Aubrey Land of the University of Maryland is Chairman of the Program Committee, and Jean Joughin of American University is Chairman of the Local Arrangements Committee.

Libraries and Archives

Containing important source material relating to the United States Weather Bureau, the personal papers of two meteorologists, Cleveland Abbe (1838–1916) and Harry Wexler (1911–1962), have been added to the Library of Congress collections.

The papers of Oswald Veblen (1880–1960), American mathematician of the Institute for Advanced Study, were presented to the Library by his wife. The documents primarily relate to the upsurge of pure mathematics and mathematical physics in America and to American and European scientists who played significant roles in this development with some material on the fellowship program of the National Research Council and the early years of the Institute for Advanced Study.

The National Archives has recently published *National Archives Accessions,* Number 57, containing a description of the records it has received during the two years from July 1, 1960, to June 30, 1962; two preliminary inventories, Number 155, *Records of the Office of the Chief Signal Officer,* and Number 156, *Records of the National Bituminous Coal Commission, 1935–36;* and Special List Number 18, *Index to Appropriation Ledgers in the Records of the Office of the Secretary of the Interior, Division of Finance, 1853–1923.*

Among microfilm publications recently completed are the US Direct Tax Lists for Pennsylvania, 1798 (24 rolls); Interior Department Territorial Papers: Arizona, 1868–1913 (8 rolls), Dakota, 1863–1889 (3 rolls), Colorado, 1861–1888 (1 roll), and Utah, 1850–1902 (6 rolls); State Department Records, 1910–1929, Relating to Internal Affairs of Chile (40 rolls), Greece (45 rolls), and Korea (45 rolls), and to Political Relations between the United States and Chile (1 roll), the United States and Greece (3 rolls), Greece and Other States (6 rolls), and Chile and Other States (2 rolls); Letters of Application and Recommendation during the Administrations of Thomas Jefferson, 1801–1809 (12 rolls), and of James Madison, 1809–1817 (8 rolls); Records of the US District Court for the District of Columbia Relating to Slaves, 1851–1863 (3 rolls); Volume XXVII (1962) of the *Federal Register* (6 rolls); Compiled Service Records for Volunteer Union Soldiers Who Served in Organizations from the Territory of New Mexico (46 rolls); Orders and Endorsements Sent by the Secretary of War, 1846–1870 (13 rolls); the Buell Collection of Historical Documents Relating to the Corps of Engineers, 1801–1819 (3 rolls); Letters Received by the Confederate Quartermaster General, 1861–1865 (14 rolls); and the Union Provost Marshal's File of Two- or More-Name Papers Relating to Citizens (94 rolls).

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The Department of State has now completed the transfer of the last allotment of microfilms of the archives of the former German Foreign Ministry to the National Archives. All microfilms of files made by the American-British-French project and listed in the appendixes of the volumes of Series C and D, *Documents on German Foreign Policy, 1918–1945,* are now available to scholars at the Archives, where the collection of films is entitled Microcopy T-120. A complete listing of all these films will be available to scholars on the completion of the guide entitled: *A Catalog of Files and Microfilms of the German Foreign Ministry Archives, 1920–1945,* Volume II, compiled and edited by George O. Kent and published as a joint project of the United States Department of State and the Hoover Institution on War, Revolution, and Peace.

The Franklin D. Roosevelt Library has received from Isador Lubin an important collection of papers relating to his government service, including that as US Commissioner of Labor Statistics. Other papers were received from Paul H. Appleby, Undersecretary of Agriculture, 1940–1944; David Gray, uncle of Mrs. Roosevelt and minister to Ireland, 1940–1947; and Henry Field, anthropologist and adviser to President Roosevelt on refugee resettlement problems.

The Harry S. Truman Library received additional papers of Frederick J. Lawton, former Director of the Bureau of the Budget, and of Frieda Hennock, former member of the Federal Communications Commission; White House files of Matthew J. Connelly; and a large, well-organized clipping file of the Democratic National Committee.

The National Historical Publications Commission met June 17, 1963, to consider plans for financing its general program as recommended in its *Report to the President . . .* , published in March 1963.

On June 18, members of the commission and others testified before a House subcommittee hearing on a bill to enact into law recommendations made in the commission's *Report.* Similar legislation (S. 1307) has been introduced in the Senate. As for the recommendations of the commission that involve contributions from private philanthropy for continued support of the so-called "priority projects" (the Jefferson Papers, Franklin Papers, Hamilton Papers, Adams Papers, and Madison Papers) and the proposed $500,000 a year to the commission to match a similar amount expected from congressional appropriations, a committee to represent the seven sponsoring institutions for the priority projects and the commission was formed to plan and coordinate the approach to foundations and other possible sources of support.

Micro Methods, Ltd., in conjunction with the British Association for American Studies has produced microfilm of various British records relating to America. Based on texts that were reported in Bernard Crick and Miriam Alman's *Guide to Manuscripts Relating to America in Great Britain and Ireland,* the film is being prepared in two series under the general title "British Records Relating to America in Microform." Walter Minchinton, University College, Swansea, Wales, has copies of the typed introductions. Orders for microfilms of catalogues can be placed with Micro Methods, Ltd., East Ardsley, Wakefield, Yorkshire, England.

The historical papers of George Mason, Virginia statesman, will be collected

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and edited by Robert A. Rutland in a project sponsored by the Board of Regents of Gunston Hall and the Institute of Early American History and Culture.

David H. Clift, executive director of the American Library Association, has announced plans for a new journal designed primarily to assist college and university libraries in their selection of books.

International Historical Activities

The Bureau (executive body) of the International Committee of Historical Sciences met in Warsaw, Poland, during the week of May 19. The following historians were present: A. A. Gouber, first Vice-President, who presided; Gerhard Ritter, second Vice-President; Michel François, Secretary-General; Louis Junod, Treasurer; Ramon Carande, Paul Harsin, E. F. Jacob, R. Morghen, Boyd C. Shafer, Councilors; and Robert Fawtier, past President. Professor Friedrich Engel-Janosi represented the national committee of Austria which is responsible for the next Congress. The Polish national committee was host and provided well-organized material arrangements for the meeting.

The chief business before the Bureau was to plan the program of the Vienna Congress which will be held in late August 1965.

At the Congress, the Bureau decided, the Reports will be divided into four sections, Section I being devoted to six major themes, Section II to the history of continents, Section III to the papers of the special Commissions, and Section IV to methodology and chronological history. In addition, there will be opening and closing sessions and two evening conferences. Previous decisions of the Bureau had provided that no "communications" (short papers) would be presented at Vienna, and these decisions were not changed. No final decisions were reached concerning the "interventions" (discussions).

Historians from the United States, Gordon Craig, Louis Gottschalk, Robert Kann, Hans Kohn, and Carl Schorske, will be responsible for five Reports in Sections I and IV, and other American scholars will take part in other Reports including those of the Commissions (Section III). The Reports will be published before the Congress, those of Section I in one hundred pages each, those of Section II in thirty-three pages each, and those of Sections III and IV in résumés of about ten pages each.

The subscription for historians attending the Congress will be $15.00, for members of their families $7.50, and for regularly enrolled students $5.00.

The program as now arranged consists of the Reports listed below, with the names of the responsible historians and those who will participate in the preparations of the Reports.

TABLEAU DÉFINITIF DES THÈMES DE RAPPORTS RETENUS POUR LE XII° CONGRÈS INTERNATIONAL DES SCIENCES HISTORIQUES DE VIENNE EN 1965

Ière SECTION
GRANDS THÈMES

1. *L'acculturation*
Responsable: France (A. AYMARD, Paris)

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Participants: J. Stengers (Bruxelles): L'acculturation dans l'ancien Congo belge
D. B. Quinn (Liverpool): Exploration and the Expansion of Europe
S. Mazzarino (Italie): I problemi dell' acculturazione nell'ero antico
F. Valsecchi (Roma): Il nuovo ruolo del Mediterraneo dopo l'apertura del canale di Suez
E. Condurachi (Roumanie): La Mer Noire dans l'histoire
F. Köprülü (Ankara): Orient et Occident après l'apparition de l'Islam

2. *La tolérance religieuse et les hérésies à l'époque moderne*
Responsable: Grande Bretagne (E. G. RUPP)
Participants: M. Schmidt (Mainz): Der radikale deutsche Protestantismus der Reformationszeit und die Independenten
G. Schilfert (Berlin): Der Einfluss der englischen Independenten auf die deutschen Sekten in der 2. Häfte des 18 Jh.
E. Weinzierl (Wien): Der Toleranzbegriff in der österreichischen Kirchenpolitik
J. Caro Baroja (Madrid): Minorias israelitas en el Occidente europeo durante la Edad moderna
M. Themelly (Italie): Correnti cattoliche dissidenti in Italia nell'età del Risorgimento 1815–1870
A. G. Dickens (London): England and the Reformation

3. *Nationalisme et internationalisme aux* XIX^e^ *et* XX^e^ *siècles*
Responsable: U.S.A. (H. KOHN, New-York)
Participants: K. D. Erdmann (Kiehl): Briands Europaplan in der internationalen Diskussion
G. Stourzh (Wien): Les nations de l'Europe centrale et du Sud-Est et la dissolution de l'Autriche-Hongrie
D. Kossev (Sofia): Les luttes de libération nationale dans les pays balkaniques et la politique des grandes puissances au XIX^e^ s.
P. Renvalls (Finlande): Die Entwicklung neuer nationaler Staaten d.b. Nationalismus dynastischer Staat und Internationalismus in der Neuzeit
T. Desmond Williams (Dublin): The Irish Revolutionary Movement (1916–1939), Revolution and Counter-revolution
F. Valsecchi (Roma): Correnti politiche italiana favoreli alla dissoluzione dell Austria-Ungheria 1914–1918
V. Cherestesiu (Roumanie): L'importance internationale de la révolution de 1848–1849

4. *Les classes dirigeantes de l'Antiquité aux temps modernes*
Responsables: Allemagne (G. TELLENBACH, Fribourg/Brisgau)
Grande-Bretagne (A. GOODWIN, Manchester)
Italie (R. ANDREOTTI)
Participants: L. Genicot (Louvain): Les classes dirigeantes
K. B. MacFarlane (Oxford): The English Nobility in the Later Middle Ages
O. Bertolini (Italie): Le classi dirigenti nell'Italia altomedievale dal sec. V al sec. IX
F. Kiechle (Erlangen): Die grieschische Führungsschicht im 6 bis 5 Jh.
L. Bulferetti (Genova): La nobilità del Piemonte a e Genova nel sec. XVII
C. Violante (Italie): La formazione del patriziato cattolico milanese dal X al XII secolo
B. Djurdjev (Yougoslavie): Les états, les classes dirigeantes et la révolution dans l'histoire

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5. *Structures sociales et littératures aux* XIX° *et* XX° *siècles*
 Responsables: U.R.S.S. (D. D. BLAGOY) et U.S.A. (C. E. SCHORSKE, Berkeley)
 Participants: F. Braudel (Paris): Littérature et histoire en France au XVIII° s.
6. *Bilan du monde en 1815*
 Responsable: France (E. LABROUSSE, Paris)
 Participants: N. Markert (Tübingen): Russland und die Neuordnung Europas 1815
 L. Hertzman (Alberta): Change and Continuity in the Western European Society in the Aftermath of the French Revolution
 G. S. Graham (London): The Outcome of the Napoleonic Wars in Regard to the Balance of Powers Both Commercially and Strategically
 D. Demarco (Italie): L'Europa e il mondo al tempo del Congresso di Vienna al bilancio demografico italiano
 A. Z. Manfred, A. G. Narstchnitsky (U.R.S.S.): 1815: L'Europe après 25 ans de révolutions et de guerres

II° SECTION
HISTOIRE DES CONTINENTS

1. *Le relazzioni religiose dell'Europa con l'Asia e l'Africa nel basso Medio Evo*
 Responsable: Saint-Siège (Mgr. MACCARRONE, Città del Vaticano)
 Participants: O. Halecki (New-York): Diplomatie pontificale et activité missionnaire en Asie aux XIII°–XV° siècles
 J. Beckmann (Fribourg, Suisse): Die Päpste des XV Jh. und die portugiesischen Entdeckungen in Afrika
2. *Extrême-Orient*
 Responsable: Japon
 Noboru NIIDA (Tokyo): Chinese Legal Institutions of Sin and T'ang Periods
 Sadao NISHIJIMA (Tokyo) et Tatsuo MASUBUCHI (Tokyo): Characteristics of the Unified States of Chin and Han
3. *La Décolonisation*
 Responsable: U.R.S.S. (A. A. GOUBER et A. F. MILLER, Moscou): Les changements politiques et économiques dans les pays d'Asie et d'Afrique au XX° siècle
 Participants: R. V. Albertini (Heidelberg): Dekolonisation als historisches Problem
 Miss Perham (Oxford)
 D. Rothermund (Marburg): Forschungen über die Rolle der westlichen Bildungsschichts in den politischen Massenbewegungen Indiens im 20 Jh.
4. *Le problème des sources de l'histoire de l'Afrique noire jusqu'à la colonisation*
 Responsable: France (J. GLENISSON, Paris, et MAUNY, Paris)
 Participants: Ivan Hrbek (Prague)
 W. Markov (Leipzig): Zur Periodisierung der afrikanischen Geschichte
5. *Genèse et continuité des anciennes civilisations américaines*
 Responsable: Autriche (Robert HEINE-GELDERN, Wien)
 Participants: Gordon F. Ekholm (New-York)
 Ignacio Bernal (Mexico)
 Paul Kirchoff (Mexico)
6. *La estructura político-administrativa hispánica como base de las nacionalidades americanas*
 Responsable: Espagne (J. M. OTS-CAPDEQUI, Valencia)

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III° SECTION
COMMISSIONS

1. *Association internationale des Études byzantines*	
—Le monde de Byzance dans la pensée historique de l'Europe à partir du XVII° s.	A. Pertusi S. Runciman D. Obolensky I. Dijcev D. Zakythinos
2. *Commission internationale des Études slaves*	
—La place dans la conception de l'histoire mondiale de l'histoire du monde slave et de l'Europe orientale	
3. *Association internationale d'histoire du droit et des institutions*	
—L'organisation judiciaire: Fondements, structure juridique, réalités sociales	
4. *Commission internationale d'histoire économique*	
—Taux d'intérêt et investissements depuis le Moyen Âge	Berril (Grde Bretagne)
—Paysage et peuplement rural après le XI° s.	Le Goff-Philippe (France)
5. *Instituto panamericano de Geografía e Historia*	
—La independencia de los paises de la América latina	R. Humphreys (London)
6. *Fédération internationale des Instituts d'histoire de la Renaissance*	
—Humanisme et réformation	
—Die humanistischen Erzichungsschriften, Textforschung und Menschenbild	O. Herding (Münster) H. Baron (Chicago)
7. *Commission pour l'histoire des Assemblées d'État*	
—Representative Assemblies and Taxation	E. Lousse (Louvain)
8. *Commission d'histoire urbaine*	
—Évolution de l'historiographie urbaine en Europe	F. Vercauteren (Liège)
—Die Stadtplanforschung	H. Amman (Aarau)
9. *Commission internationale d'histoire des mouvements sociaux et des structures sociales*	
—La participation populaire aux mouvements nationaux d'indépendance aux XIX° et XX° siècles	D. Demarco J. Dhondt Mme Fauvel-Rouif
10. *Commission internationale d'histoire ecclésiastique comparée*	
—La vie religieuse à la fin du XV° et à la fin du XVI° siècle	
11. *Commission internationale d'histoire maritime*	
—Liaison et concurrences des voies maritimes et continentales dans le commerce international, depuis le XV° s.	J. Heers (Caen) H. Kellenbenz (Köln)
—Bibliographie de l'histoire des grandes routes maritimes	Ch. Verlinden (Roma)
12. *Commission internationale d'histoire de la presse*	
—L'influence des moyens de diffusion collective sur les évènements historiques et sur l'évolution politique	V. Klimes (Prague) G. Gaeta (Trieste)
—La Presse et les révolutions du XIX° s.; le cas de la Belgique	Th. Luyky (Gand)
—La presse américaine et canadienne dans la décolonisation	Nelson (Madison)
—Les nouvelles à la main	J. Godechot (Toulouse)
—Economics as a Government Weapon for Control of the Press	Nelson (Madison)

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13. *Commission internationale pour l'histoire des Universités*
—Le rôle politique des universités au Moyen Âge et au XVI° siècle
14. *Commission internationale de démographie historique*
—Les migrations du XVII° au milieu du XIX° s. Krause (Rutgers)

IV° SECTION
MÉTHOLOLOGIE ET HISTOIRE CHRONOLOGIQUE

I. *Méthodologie—Histoire générale—Historiographie*
1. Projects and Concepts of World History in the xxth Century
Responsable: U.S.A. (L. GOTTSCHALK, Chicago)
2. Evolution und Revolution in der Weltgeschichte
Responsable: Allemagne DDR (E. ENGELBERG, Berlin)
Participants: Bulgarie (C. Christov, Sofia)
Roumanie (D. D. Rosca, Cluj)
Tchécoslovaquie (M. Hubl, Prague)
3. Federalism and Federal State in History
Responsable: U.S.A. (R. A. KANN, Rutgers)
4. Mongolian Historiography
Responsable: Mongolie Extérieure
5. L'évolution de la pensée historique au milieu du XX° siècle
Responsable: U.R.S.S. (Mme NETCHKINA, MM. V. T. PACHOUTO et E. B. TCHERNIAK)
6. Der Eintritt der Südhemisphäre in die europäische Geschichte und das Problem des Austral Kontinents
Responsable: Autriche (G. HAMANN, Wien)

II. *Antiquité*
1. Die Zwischenstaatlichen Beziehungen der griechischen Städte im Klassischen Zeitalter
Responsable: Allemagne fédérale (H. BENGTSON, Würzburg)
2. Les mouvements sociaux à l'époque hellénistique
Responsable: Pologne (Mme MAŁOWIST, Varsovie)
Participant: Finlande (J. Suolahtis)
3. The Roots of Ancient Cypriot Civilisation and the Miceanean Colonisation
Responsable: Chypre (P. DIKAIOS)
4. Antiquité et christianisme
Responsable: Commission d'histoire ecclésiastique (J. R. PALANQUE, Aix)
5. Economic Problems in the Illyrian Area in the Late Roman Empire
Responsable: Finlande (P. BRUUNS)
6. Das Bild des Themistokles in der antiken Geschichtsschreibung
Responsable: Autriche (SCHACHERMEYR)

III. *Moyen Âge*
1. La genèse des États en Europe centrale au Moyen Âge
Responsable: Tchécoslovaquie (F. GRAUS, Prague)
Participants: Pologne (A. Gieysztor, Varsovie)
Roumanie (C. Daicoviciu et E. M. Petrovici)
2. Die Rolle der Araber in der Überlieferung des antiken Geistesgutes
Responsable: Allemagne fédérale (F. ALTHEIM, Berlin)
Participant: Espagne (M. Millà, Barcelone)
3. Zum Problem der Schlichtung von Streitigkeiten im Mittelalter
Responsable: Allemagne fédérale (J. ENGEL, Tübingen)
4. Conceptions et méthodes de l'histoire des villes médiévales au cours du dernier demi-siècle
Responsable: Belgique (P. BONENFANT, Bruxelles)

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5. Die Rechtstradition in den mittelalterlichen Papsturkunden
 Responsable: Autriche (G. ZIMMERMANN, Wien)
6. Die Môglichkeiten weiterer Entwicklung der Diplomatik im Rahmen der historischen Mediävistik
 Responsable: Tchécoslovaquie (J. SEBANEK, Brno)

IV. *Histoire moderne et contemporaine*

1. Les bases économiques et sociales de l'absolutisme
 Responsable: Hongrie (E. MOLNAR, Budapest)
 Participants: Allemagne fédérale (H. Kellenbenz, Köln): Problem der Merkantilismusforschung (avec la collaboration de M. Hassinger, Wien, et L. Herlitz, Stockholm)
 Allemagne DDR (G. Heitz, Rostock): Der Zusammenhang zwischen den Bauernbewegungen und der Entwicklung des Absolutismus im Mitteleuropa
 Autriche (H. L. Mikoletzsky, Wien): Die Anfänge der Industrie und der Staatsfinanzen in Österreich im 18 Jh.
 Autriche (H. Sturmberger, Linz): Das Problem der Vorbildhaftigkeit des türkischen Staatswesens im 16 und 17 Jh. und sein Einfluss auf den europäischen Absolutismus
2. Mouvements paysans du Centre et du Sud-Est de l'Europe du xv° au xx° s.
 Responsable: Roumanie (S. PASCU, Cluj)
 Participants: U.R.S.S. (V. V. Mavrodine, B. F. Porchnev, J. G. Ankelava)
3. Les efforts des peuples des Balkans pour accéder à l'indépendance politique et économique de 1875 à 1914
 Responsable: Yougoslavie (D. DJORDJEVIC et M. EKMECIC)
4. Problèmes économiques et sociaux de la I^{ère} guerre mondiale
 Responsable: U.R.S.S. (A. L. SIDOROV et K. B. VINOGRADOV)
5. Problèmes politiques da la I^{ère} guerre mondiale
 Responsables: Allemagne fédérale (G. RITTER): Die politische Rolle Bethmann Hollwegs während des Ersten Weltkriegs
 Autriche (F. ENGEL-JANOSI): Die Friedensbemühungen Kaiser Karls
6. The Impact of the German Military on the Political and Social Life of Germany during World War II
 Responsable: U.S.A. (GORDON CRAIG, Stanford)

Séance d'ouverture

"Die Spätmittelalterlichen Universitäten Europas" et adresse de K. LEPSZY (Cracovie): "Salut de l'université de Cracovie à l'université de Vienne"

Séance de clôture

E. ZÖLLNER (Wien): "Die Kulturelle Ausstrahlung Wiens" et adresse de V. L. TAPIÉ (Paris): "La place de Vienne dans la culture européenne"

Conférences du soir

1. Th. SCHIEDER (Köln): "Der Nationalstaat in Europa als historisches Phänomen"
2. J. DROZ (Paris): "Sozialismus and Nationalitätsfrage am Ende des 19. Jahrhunderts."

The thirteenth All-Pakistan History Conference was held at Lahore, April 9–11, 1963. Sh. Abdur Rashid of the University of Punjab was responsible for the meeting. Gilbert Fite discussed the "Teaching of American History in the Subcontinent," and John Shover, who represented the American Historical Association, spoke on "Recent Developments in Historiography in the United States."

The First International Congress on the Enlightenment, organized by the *Institut et Musée Voltaire* under the auspices of the state and city of Geneva, was

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held at Geneva and at the Château de Coppet, July 5–12. Those in attendance came from the United States, Switzerland, France, Great Britain, Belgium, Holland, West Germany, East Germany, Canada, Australia, New Zealand, Denmark, Norway, Sweden, Italy, Greece, Rumania, Hungary, Poland, and Japan. The transactions of the congress are to be published in the "Studies on Voltaire and the Eighteenth Century" ("Publications de l'Institut et Musée Voltaire"). A committee, of which Adrienne Koch (California) and Harcourt Brown (Brown) are the American members, was elected to decide upon the time and place of the next congress.

Expansion of the use of binational commissions in other countries, to assist the development of American studies overseas as well as the general academic exchange program of the United States, was recommended in a recent report forwarded to Congress by the US Advisory Commission on International Educational and Cultural Affairs. The report, prepared by Walter Johnson, professor of American history at the University of Chicago and member of the commission, proposed that such binational commissions as now operate in forty-four countries be expanded to as many as possible of some seventy other countries with which the Department of State conducts exchange programs. Johnson urged greater care in choosing foreign institutions for placing American scholars and establishing chairs of American studies, giving preference to universities that would incorporate American studies into their required curriculum, draw visiting scholars into the full professional life of the institution, and provide instruction in American studies by their own faculty members after an initial period. Since foreign currency funds became available to the Department of State for chairs and workshops in American studies, chairs in American history and institutions have been established at the University of Bombay and Jadavpur University in Calcutta.

Grants, Awards, Prizes

With $500,000 grants from the Ford Foundation, the Universities of Kansas and Oregon and Washington University plan to strengthen existing foreign area programs. At Kansas, a major portion of the grant will be applied to Latin American, Slavic, and East Asian studies. Oregon will broaden its programs on Japan and Latin America and on the study of overseas technical assistance. It will also attempt to develop closer relations with six other state-supported institutions in the international field. Washington will improve its library resources, make additions to the faculty, and support faculty research.

The Ford Foundation has given the African Studies Association a grant of $90,000 for the compilation of archival materials relating to Africa and the exchange of such materials with the United States, Britain, and Africa.

The American Council of Learned Societies has awarded grants-in-aid for 1963–1964 to the following historians: Donald E. Chipman, Ann Freeman, Astrik L. Gabriel, Michael G. Hall, Charles W. Hollister, Gabriel Jackson, Robert A. Kann, Benjamin W. Labaree, Lester K. Little, Irwin L. Merker, James E. O'Neill, Ruth Pike, and Philip J. Staudenraus.

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Among European scholars who received ACLS fellowships for study and research in the United States for 1963–1964, or renewals of fellowships previously granted, are: Guillermo Céspedes del Castillo, N. Göran Rystad, Klaus G. A. Schwabe, Andrew A. Sinclair, Lawrence J. Williams, and Dragoljub Zivojinović.

Joint committees of the American Council of Learned Societies and the Social Science Research Council have awarded the following Foreign Area Fellowships for 1963–1964: *African Studies Program*—Ralph A. Austen, Victoria H. Bomba, Prosser Gifford, Robert R. Griffeth, George W. Johnson, Jr., Raymond K. Kent, Victor N. Low, Marian E. McReynolds, Lawrence P. Ralston, and Leo Spitzer. *Asia and Near East Program*—Gail L. Bernstein, Philip B. Calkins, John J. Donohue, S. J., George S. Elison, Peter K. Frost, Leonard Gordon, David Hamilton, George C. Hatch, Jr., Thomas R. H. Havens, Philip Chung-Chi Huang, Ronald B. Inden, Robert L. Irick, Byron K. Marshall, Lion G. Miles, Peter MacV. Mitchell, George E. Moore, Ray A. Moore, Virgil D. Morris, Jr., William A. Oates, James B. Palais, Don C. Price, Sharon L. Sievers, Kent C. Smith, William M. Speidel, Glen W. Swanson, Philip L. Thompson, Richard P. Tucker, Abraham L. Udovitch, Frank D. Van Aalst, John O. Voll, Frederic E. Wakeman, Jr., Constance M. Wilson, Martin G. Wolfson, and Ernest P. Young. *Latin American Studies Program*—Frederick P. Bowser III, Ralph S. della Cava, Ronald H. Dolkart, Peter G. Marzahl, William R. Svec, and John D. Wirth. *Soviet and East European Studies*—John T. Alexander, Michael W. Curran, Daniel Field, Charles A. Frazee, Joseph T. Fuhrmann, Richard J. Hellie, John F. Hutchinson, Alan S. Lichtenstein, George P. Majeska, James C. Mills, Jr., Piroska Molnar, Thomas G. Pesek, George W. Phillips, Philip Pomper, Alexander Rabinowitch, Paul L. Roley, Mary E. Schaeffer, Arnold Schrier, Marshall S. Shatz, Judith D. Stewart, Ronald G. Suny, Benjamin P. Uroff, Richard S. Wortman, and Judith E. Zimmerman.

The Joint Committee on Contemporary China, also sponsored by the ACLS and the SSRC, awarded grants for research to Joseph J. Lee and Karl A. Wittfogel.

The ACLS and the SSRC jointly offer a limited number of fellowships for the year 1964–1965 for graduate training in the social sciences and the humanities relating to Asia and the Near East, the Soviet Union and Eastern Europe, Africa (south of the Sahara), and Latin America. These fellowships are intended to support the combination of multidisciplinary area training with the spoken and written language of the area and with training in a major discipline. Programs may be undertaken in the United States or abroad, or a combination of the two. Application forms and further information may be obtained from the Foreign Area Fellowship Program, 444 Madison Avenue, New York 22, New York.

The Rockefeller Foundation has awarded a grant to Daniel F. McCall of Boston University to investigate archival material on Africa at universities, museums, and archives in Europe and North Africa.

John McV. Haight, Jr., has been awarded a grant by the American Philosophical Society for research and study in France and England.

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The Society of American Historians, Inc., announced that the sixth annual Parkman Prize was awarded to James T. Flexner for his *That Wilder Image,* and the second Allan Nevins Prize for the best-written doctoral dissertation in American history was awarded to Mrs. Willie Lee Rose for her "Rehearsal for Reconstruction." Entries and inquiries concerning the 1963 prize should be sent to Professor John A. Garraty, Society of American Historians, Fayerweather Hall, Columbia University, New York, New York.

LeRoy H. Fischer has won the five-thousand-dollar National Literary Award Competition of the Loyal Legion of the United States for the best book-length manuscript on the American Civil War, entitled "Lincoln's Gadfly Adam Gurowski."

David L. Hoggan received the Leopold von Ranke Prize of the *Gesellschaft zur Foerderung Geschichtswissenschaftlicher Forschung e. V.* for his *Der Erzwungene Krieg.*

Publications

The first volume of *Explorations in Entrepreneurial History,* Second Series, appears this fall. Further information and subscription rates can be obtained from Ralph Andreano, Editor, Earlham College, Richmond, Indiana.

Other Historical News

The Mississippi Valley Historical Association has chosen Avery O. Craven to be President, John Caughey, Vice-President, and Harvey Wish, James C. Olson, and C. Vann Woodward as elected members of its executive committee. The 1964 meeting will be held at Cleveland, Ohio, April 30–May 2.

The Fifth Missouri Conference on History was held at the University of Missouri on March 29–30. The principal speaker, Crane Brinton, presented a paper on "Reflections on French Revolutions."

Recent Deaths

Andrew Wallace Crandall died May 1, 1963, at the age of sixty-eight. Having graduated from Central College, he obtained an M.A. degree from the University of Chicago and the Ph.D. degree from the University of Pennsylvania. He taught at DePauw from 1921 until his retirement in 1960, when he continued part-time teaching as Eugene C. Pulliam Professor of American History. *The Early History of the Republican Party* was his major work.

Ralph Simpson Kuykendall, professor emeritus of history at the University of Hawaii and dean of Hawaii historians, died May 9, at the age of seventy-eight. Kuykendall graduated from the College of the Pacific in 1910 and worked for the California Historical Survey Commission before receiving his M.A. from the University of California in 1918. After serving as the late H. E. Bolton's as-

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sistant at Berkeley until 1921, he went to Hawaii the following year as executive secretary of the Hawaiian Historical Commission, a territorial agency charged with writing a secondary school history, a history of Hawaii's part in World War I, and a definitive history of the monarchy. When the work of the commission was transferred to the University of Hawaii, he joined the faculty as assistant professor of history. He retired as professor emeritus in 1950; the university awarded him the honorary degree of doctor of humane letters in 1956. He continued his research and writing after retirement from teaching and at his death was within twenty pages of completing the last chapter of the third volume of his meticulous history of the *Hawaiian Kingdom*. The volume will be completed from his notes. In addition to writing scores of articles, he willingly shared his time and knowledge. He was a member of the American Historical Association from 1918 and of the Pacific Coast Branch from its beginning.

Cornelius D. Penner, professor and head of the department of history at Baldwin-Wallace College, died May 18, at the age of sixty-four. He had been a member of the faculty there since 1940.

Sir Maurice Powicke, the leading English medievalist of his time, died May 19, at the age of eighty-three. The son of The Reverend F. J. Powicke, principal authority on Richard Baxter, Powicke inherited the interest in religious thought and history that is found predominantly in his work. His early schooling was at Stockport, then at Owen's College, Manchester, where he came under the influence of Professor T. F. Tout and of his colleague James Tait. From Manchester he went to Balliol College to work on the history of Normandy in the twelfth and early thirteenth centuries. In 1906 he returned to the history department in Manchester, but in 1908 was elected a fellow of Merton and the next year became professor of modern history at Belfast. His first major work was *The Loss of Normandy 1189–1204,* which appeared in 1913 (2d ed., 1961). This was an exploration of Anglo-Norman history from Norman records, an attempt to show how the history of England under the Angevins was modified and assisted by an examination of Norman evidence. In 1919 he became professor of medieval history at Manchester and turned to his studies of the moral and intellectual foundations of English thirteenth-century history in a number of publications, the first of which was his book on Stephen Langton ("The Ford Lectures, 1928"). Meanwhile he was working on the reigns of Henry III and Edward I, full results of which were his *Henry III and the Lord Edward* (1947) and the thirteenth-century volume of the "Oxford History of England" (1953; 2d ed., 1962). There were other volumes of papers and collected essays in which he has revealed, as has been well said, "the springs of action and thought in the Middle Ages."

He was one of the most prolific of English medievalists, but all he did was touched with critical insight and warm human appreciation. His friends and pupils were numerous; his contacts with scholars in the United States firm and abundant. He was an honorary member of the American Historical Association

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and a doctor of Harvard where he lectured at the Tercentenary Congress of Arts and Sciences. At one time he spent some months working in the Huntington Library.

Dorothy Mackay Quynn, long a member of this Association, died on May 21.

Waldemar Westergaard, "Westy" to the initiated, justly celebrated as a prince among men, died June 4, at the age of eighty-one. Death came unexpectedly, for it was life which absorbed this pioneer teacher and historian. Born in North Dakota of Danish emigrant parents, Westergaard rose from teaching in country schools to play an important role in the development of Pomona College and the young University of California at Los Angeles. The impact of his leadership in establishing standards extended well beyond the departments of history that he headed. The whole academic community, numerous societies, and the nation received his quiet services. As a historian, meanwhile, Westergaard pioneered in making possible serious study of Scandinavian and Baltic history in America. He became a living bridge between America and Northern Europe. His numerous lifelong friendships abroad, his collections of vital source materials, the students he trained, and his respectable list of publications bear witness to his activity. So also do the honors bestowed upon him (including knighthood in Denmark). It is characteristic of his chosen field, however, and of the man who dominated it, that his greatest contribution was made through his personal relationships and unflagging correspondence rather than through the printing press. He will not soon be forgotten.

William Toth, newly elected executive director of the Foundation for Reformation Research, St. Louis, died June 21, at the age of fifty-eight. He studied at Franklin and Marshall College, Lancaster Theological Seminary, and the University of Budapest, securing his Ph.D. degree from Yale University in 1941. Appointed to a professorship at Franklin and Marshall College in 1946, he was interested in the history of the Reformation in Hungary, having published several articles in *Church History* and the *Archiv für Reformationsgeschichte,* and in the history of the Evangelical and Reformed Church in America.

Communications

To the Editor of the American Historical Review:

Book reviews that are critical but accurate render a service both to the author of the book reviewed and to the potential reading public. Reviews, on the other hand, that suggest and imply vague failings instead of fastening on facts cannot be accorded the respect that a greater amount of spadework on the part of the reviewer could have elicited. Professor Rustow's remarks on my *Genesis of Young Ottoman Thought* (*AHR,* LXVIII [Apr. 1963], 750) fall within this second category.

To state, for example, that "one suspects" that tertiary and quaternary sources were used in the book is the very opposite of the approach one would expect from a scholar. In addition, this sentence has a peculiar boomerang effect in that while it casts an invidious shadow on the work reviewed, it at the same time

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raises doubts about the reviewer's command of Ottoman sources. A similar example of unfairness may be seen in the expression "a questionable 'reconstruction' at third hand of the 'immediate origins' of the Gülhane *Hatt* of 1839." For one, the use of the adjective "questionable" without further explanation introduces into the argument an imputation the net value of which—in terms of clarifying what was attempted by the author—is nil. Secondly, your reviewer does not seem to be aware that the "reconstruction" alluded to is based on articles and books that use primary sources. I would be grateful if he could indicate other sources available at present that would allow one to describe the intellectual influences that went into the shaping of the *Hatt* of 1839. The use of quotation marks around the word "reconstruction" is, again, an attempt to substitute typographical one-upmanship for serious criticism. A number of similarly constructed sentences may be found in the review that create the impression that peevishness is used as a substitute for textual criticism.

When your reviewer states that "no attempt was made to investigate what proportion" of the bureaucrats, the ulema, and the military who sided with the Young Ottomans, he is either out of his depth or is imposing on the author a task that he well knows would take a number of years and several monographs to clarify. As laudable as Professor Rustow's attempt to Namierize Turkish history may be, his methodological strictures have no relevance to the task attempted in my book. On the other hand, he will find my conclusions regarding the *küttab* amply supported by the findings of Professor Halil İnalcık in his article "Reis ül-Küttab" about to appear in the *Islâm Ansiklopedisi*.

Finally, to quote two sentences on the ulema out of context so as to make them seem contradictory cannot be accepted as a fair assessment of the question of the ulema as it is taken up in the book. What could have been pointed out, in this respect, is that the remarks made with regard to the ulema in the *Genesis of Young Ottoman Thought* have been to a considerable extent corroborated by Uriel Heyd in his article on the Ottoman ulema, which appeared while my book was in the press (see "The Ottoman 'Ulema and Westernization at the Time of Selim III and Mahmud II," *Scripta Hierosolymitana,* IX [1961], 63–96, and particularly p. 78 on the decline of the ulema in the eighteenth century).

The asperity of that part of the review by Professor Rustow which takes up the portions of my book analyzing the *Tanzimat* is not characteristic of his normally levelheaded style. It no doubt may be explained by the difficulty of both writing about and passing judgment on the *Tanzimat* at the present stage of *Tanzimat* studies.

University of Ankara ŞERIF ARIF MARDIN

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

"Invidious shadows"? "Peevishness"? "Typographical one-upmanship"? I am pained—and puzzled—that Dr. Mardin should have deduced these effects from my review. I can assure him that none of them were intended. (I employed quotation marks to indicate words or phrases used by the author himself; I do so again.)

Dr. Mardin's original research is excellent, but its presentation is marred by a tendency to digression and to overstatement. These detract from the unity and persuasiveness of his book. In several chapters the author strays far from his chosen theme into topics that (as we both now agree) he could not have been expected to explore adequately. And frequently he resorts to categoric statements

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not easily susceptible of proof ("completely corrupt and ignorant," p. 142) or presents as firm conclusions what more properly are suggestive hypotheses. We are agreed that his analysis of the *Hatt* of 1839 is based on nonprimary sources, yet his own summary of that passage ("a reconstruction of the events just prior to its proclamation," p. 169) suggested—at least to me—an interpretation based on fresh material. Similarly, Mardin's data show that, within the Young Ottoman movement, *some* lower bureaucrats were allied with *some* ulema and *some* military officers; instead he attributes the conclusion of "this triple alliance of bureaucrats, ulema, and soldiers" to developments impinging on *"the* low-ranking bureaucrats, on the *majority* of the ulema, and on the army *as a whole"* (pp. 121 ff.; italics mine). This latter proposition, I quite agree, was beyond the scope of his research, although something less than the lifetime labors of a Namier might be required to examine it empirically.

Let us step out of the shadows of sensitivity and put aside the typographic boomerangs. Mardin's work is an important and most welcome contribution to the long-neglected field of nineteenth-century Ottoman history. Together with all serious students of that field, I look forward with keen anticipation to the results of his more recent researches on the political thought of the Young Turks in exile.

Columbia University DANKWART A. RUSTOW

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

Unlike his review of *Der Erzwungene Krieg* in the October 1962 issue of the *American Historical Review,* Professor Weinberg's related letter in the April issue does contain one incontestably valid point of criticism concerning inadequate documentation for one of the important events of September 2, 1939, namely, the German reaction to the Italian proposal for a peace conference. But this same point of criticism, valuable as it has been, is not original with Weinberg. It was brought forward by Chrysostomus Zodel, editor of the *Stuttgarter Nachrichten,* in the July 28, 1962, issue of his newspaper. My reply to his point was published in the same newspaper on September 29, 1962. The matter has also been dealt with in the text for the American edition scheduled to appear this year.

The other points raised by Weinberg in his letter consist either of broad allegations too general to be of use, or of linguistic interpretations, as in the case of my comment about the situation as a whole concerning the Soviet Union and Lithuania in September 1939. Suffice it to say that the general context of Soviet intervention against Poland, which was in progress throughout the latter part of September 1939, is a very different thing from a reference to a precise event limited to September 17, 1939.

Therefore, despite the eloquence, I was unable to profit from Weinberg's review or from his letter. He still remains for me the reviewer who has failed to make the contribution we might ordinarily have expected from him. So far as his inexplicable and tasteless defamation of my German publisher, Professor Grabert, is concerned, it is my understanding that the latter has written an adequate letter of reply. My own major concern at this point is that most of the book reviews published in the *American Historical Review* do in fact contain worthwhile contributions, but that such has not been the case with Weinberg's treatment of *Der Erzwungene Krieg.*

Menlo Park, California DAVID L. HOGGAN

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To the Editor of the American Historical Review:

I have read the observations of Professor Gerhard L. Weinberg in the April 1963 issue of the *American Historical Review* relative to my comments on his review of Dr. Hoggan's book.

I do not regard it as my function to deal with details in Hoggan's text; that must be left to him. If he cannot take care of that matter, it is just too bad for him and history. I have confidence that he is quite able to take care of these matters himself. But I must point out the deliberate distortion of the most important item in my communication.

Professor Weinberg states that I merely assert "that others, four drafts and fourteen years ago, allegedly did not find the same errors and fabrications." As to anything that happened fourteen years ago, I only stated that Hoggan received his doctorate in 1948 at Harvard on a dissertation that dealt with the same general theme as his book.

It was perfectly clear from the context that any expert reading that I mentioned began, not fourteen years ago, but in 1957 after the dissertation had been expanded during some nine years into a substantial historical volume of nearly nine-hundred manuscript pages. So far as I am aware, only four persons besides myself read this first version.

The many rigorous expert readings that I mentioned were done on later drafts, the most intensive being on one that had been extensively revised *after* the German edition was published. The readers were instructed to give special attention to errors of both fact and interpretation. Since I did not mention their names in my letter, I have not exploited them in any effort to exclude errors or inventions on the part of Dr. Hoggan, although all of the readers were "reputable scholars" and specialists in the field covered by the Hoggan book. They may not have uncovered all the errors, but they were not examining an unrevised doctoral dissertation of fourteen years earlier. There has been no attempt to demonstrate "innocence by association," and nobody can agree more fully than I do that Dr. Hoggan must submit his book to "the court of historical judgment." But I hope that it will be a court somewhat more objective than the one that sat at Nuremberg.

If I devoted a little space to examining Dr. Hoggan's academic pedigree, I deemed it highly relevant in determining the relative probability as to whether he or his critics were most likely to engage in distortions, inventions, and fabrications.

Malibu, California Harry Elmer Barnes

To the Editor of the American Historical Review:

A copy of David L. Hoggan's *Der Erzwungene Krieg,* which was the subject of several letters in the April issue of the *Review,* has only recently come to my hand.

From the statement attributed to Hoggan in the publisher's foreword, and from Harry Elmer Barnes's profuse letter to the *Review,* it is clear to me that the strategy of the "revisionists" is to stimulate interest in Hoggan's book and to give it authority by suggesting that a number of American historians supported or approved it, and in fact made "a substantial contribution to it" (Hoggan).

I am given the place of honor at the head of the list, no doubt because Hoggan took his doctorate under my direction some fifteen years ago and because I, along

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with the late Professor Michael Karpovich, accepted his dissertation on German-Polish relations as meeting the requirements of the degree.

Barnes alleges that "all the main conclusions of the book reviewed were embodied in the dissertation," and implies that approval of the dissertation is tantamount to approval of the main conclusions of the book. Actually the dissertation was, if my memory does not fail me completely, no more than a solid, conscientious piece of work, critical of Polish and British policies in 1939, but not beyond what the evidence would tolerate. In any event, neither I nor my colleagues of "the most disinguished history department in the world, and surely one of the most vehemently anti-German and pro-British" would subscribe to the strange Barnesian notion that a dissertation, to be acceptable, must conform to the views of the professors. On the contrary, we are reluctant to accept work for so advanced a degree in the absence of some indications of independent thought and judgment.

I hope you can give me space to review briefly my past relations with Hoggan. Since he received his degree in 1948, we have corresponded only spasmodically, chiefly on the various occasions when he was seeking a new position and soliciting recommendations. When I learned in 1956 that he was working on the present book under Barnes's auspices, I was sorry because I knew full well what was involved. Barnes had savagely attacked *The Challenge to Isolation,* which I wrote in collaboration with S. Everett Gleason, and had not hesitated in this connection to impugn my integrity as a scholar. Having himself underlined the extent of the divergence between my views and "the truth," he had the presumption, in 1961, to appeal to me for help in finding a publisher for the Hoggan book, which by then had been turned down by various American publishing houses, despite the lure of a generous subsidy. My reply to this appeal was that I would have nothing whatever to do with the book.

I have, in fact, never in any way encouraged the author in writing this book; nor did I at any time see even a single page of the manuscript. My views on the origins of the Second World War remain what they were when I published *The Challenge to Isolation.* I not only do not subscribe to the thesis advanced in the Hoggan book; I expressly repudiate it. Furthermore, I decline responsibility for what former students of mine may write years after they have completed their training, and I regard the attempt to associate me with the Hoggan book or other writings of the "revisionist" school as unethical and reprehensible.

Harvard University WILLIAM L. LANGER

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

In the April 1963 issue of the *Review* there was printed a letter from Harry Elmer Barnes that contained the following sentence: "Moreover, when Professor Raymond J. Sontag of the University of California received an extended leave of absence to work on the documents seized from Germany after the war, he chose Hoggan as his substitute from the scores of historians available and eager for this highly honorific appointment."

Dr. Hoggan was not connected with the University of California in any capacity during the years (1946–1949) when I was editing the captured German documents. I never at any time recommended the appointment of Hoggan to the faculty of the University of California.

University of California, Berkeley RAYMOND J. SONTAG

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To the Editor of the American Historical Review:

Professor Langer's letter is such an amazing mélange of casuistry and misstatements that I can only deal adequately with it in the foreword that I shall write to Dr. Hoggan's book. Most of its distortions should, however, be crystal clear to anybody who reads carefully my letter in the April *Review*.

Actually, the American revisionist "strategy" has been to deplore any publicity in regard to Hoggan's book until it can be published in English and readers can arrive at an informed verdict as to the validity of any attacks made on it.

Neither Hoggan nor myself, so far as I can recall, has ever mentioned publicly the name of even one prominent American historian who has read and approved the Hoggan book.

Langer has never been in any way directly identified with the Hoggan *book*. To save him embarrassment, he has never been asked to read the manuscript with my knowledge or consent.

At the most, I have only indicated that the fact that the Hoggan dissertation of 1948 was approved by Langer and Professor Karpovich made me accord it respectful attention, despite its disagreement with all my previous convictions as to Hitler and the outbreak of World War II. If this seems unethical and contemptible to Langer, I am sorry even though I cannot fathom any logical reason for his intense annoyance because I have shown respect for his reputation as a scholar.

Langer's whole discussion of the dissertation item is such a morass of casuistry as to call for comment, if by anyone, from his associates in the Harvard graduate faculty of history. I would enjoy the comment of a man like Charles Homer Haskins on this paragraph.

I never "appealed" to Langer for aid in publishing Hoggan's book through the Harvard University Press or any other publisher. Langer's innuendo that Hoggan's manuscript has been kicking around in disgrace for years in the offices of the major American publishers is not true. In the light of what apparently lies ahead, we have found the ideal publisher for the book. The "generous subsidy lure" was actually the suggestion of possible substantial assistance in promoting the book. I believe that anybody familiar with the conditions under which the Langer and Gleason volumes were prepared will agree that there are few persons from whom any slur about subsidies could come with worse taste than from Langer.

If Langer wished to repudiate Hoggan's conclusions, the time to do so was in 1948, not in 1963, and he would also have logically been required to repudiate his own brilliant article on "When German Dreams Come True," published in the *Yale Review* in 1938.

For geographical and professional reasons Langer and Hoggan have not been visiting back and forth like neighbors, but they have remained on cordial personal terms. Hoggan has not needed Langer's support after the latter wrote a strong letter of commendation for Hoggan's placement agency many years ago.

Why Langer may well have been "sorry" to learn that I was having anything to do with Hoggan's book I explained very fully in my "Court Historians" brochure. Even there, I did not "savagely" attack or impugn Langer's scholarship. Quite to the contrary, I praised his scholarship, both as a diplomatic and a court historian, more highly and profusely than anybody else of whom I have any knowledge.

I am quite willing to concede that the second paragraph of Professor Sontag's

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letter is technically correct. During his long leave of absence in the 1950's he was employed by the Central Intelligence Agency rather than continuing his work on the German documents as I had been told.

Dr. Hoggan was chosen to take over his work by Professor John D. Hicks, then chairman of the history department. He had the same range of choice from a large roster of available candidates that I mistakenly assigned to Professor Sontag. Avoiding all invidious comparisons, I believe that informed persons will agree that an appointment by Professor Hicks was just as honorific as one by Professor Sontag.

Unless there was serious friction within the department, of which I have no knowledge, no substitute for a full professor over a protracted period of absence would be chosen without his consent.

Malibu, California HARRY ELMER BARNES

In accord with established rules, the *Review* will not publish further communications concerned with the review in question.

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

In his review of my *Friedrich Engels und die britische sozialistische Bewegung, 1881–1895,* in your April 1963 issue Daniel F. Calhoun writes that the book "is marred by egregious factual confusions." He cites three "examples":

The first is that "the author misinterprets the Irish demand for Home Rule as a demand for independence." There is not a single sentence in my book that could justify such an assertion. Apart from this Calhoun should be aware that the Irish rank and file during the 1880's on the whole stood for independence, and so even did a fraction of the Home Rulers. Thus, P. S. O'Hegarty, for instance, in his profound *History of Ireland under the Union* (London, 1952), writes that "Parnell and Davitt and the people all wanted national independence" (p. 513). For many of the Parnellites, Home Rule was merely a step toward complete separation.

The second charge is that I treated "the 'Great Depression' of the 1880's as if it were economically equivalent to the Great Depression of the 1930's." Is Calhoun aware that the whole matter is still being debated? If he opines that I have overstated the importance of the depression, he should say this in a manner that may be of some use for that debate.

Thirdly, he writes I was "unaware of the Fabian Society's 1893 manifesto urging Trades Union Congress sponsorship of labor candidates unattached to the Liberal party organization." I did not consider it necessary to mention this manifesto. Moreover, already in 1892 the Fabians had issued an election manifesto to the same effect. But these documents were only temporary reactions to the rising movement for the political independence of Labour. Thus, to the socialists of 1892–1893 the Fabians were "permeators" (their tactics were "to permeate Liberalism with Socialism"), which is clearly to be seen from the broad discussion on "Independence or Permeation" (to take the headline of an article in *The Workman's Times* of December 17, 1892), preceding the foundation conference of the Independent Labour party in January 1893. The 1893 manifesto was issued after the Fabians had lost nearly all provincial members, and the branches that remained were often critical of the continued "permeation" tactics. If Calhoun had written that I had done better to pay regard to the manifesto in *this* sense, I would agree with him.

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He states categorically that "as a contribution to English history, the book offers nothing new." To my knowledge, at least a number of facts in the chapters dealing with the May Day movement and with the formation of the Second International are not to be found in any books or articles.

Humboldt-Universität Berlin SIEGFRIED BÜNGER

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THE AMERICAN HISTORICAL ASSOCIATION

Founded in 1884 Chartered by Congress in 1889
Office: 400 A Street, S.E., Washington, D. C. 20003

MEMBERSHIP: Persons interested in historical studies, whether professionally or otherwise, are invited to membership. Present membership ca. 10,700. Members elect the officers by ballot.

MEETINGS: An annual meeting with a three-day program is held during the last days of each year. Many professional historical groups meet within or jointly with the Association at this time. The Pacific Coast Branch holds separate meetings on the Pacific Coast and publishes the *Pacific Historical Review.*

PUBLICATIONS AND SERVICES: The official organ, the *American Historical Review,* is published quarterly and sent to all members. It is available by subscription to others. In addition, the Association publishes its *Annual Report,* prize monographs, pamphlets designed to aid teachers of history, bibliographical as well as other volumes, and a newsletter. To promote history and assist historians, the Association offers many other services. It also maintains close relations with international, specialized, state, and local historical societies through conferences and correspondence.

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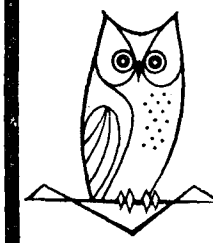
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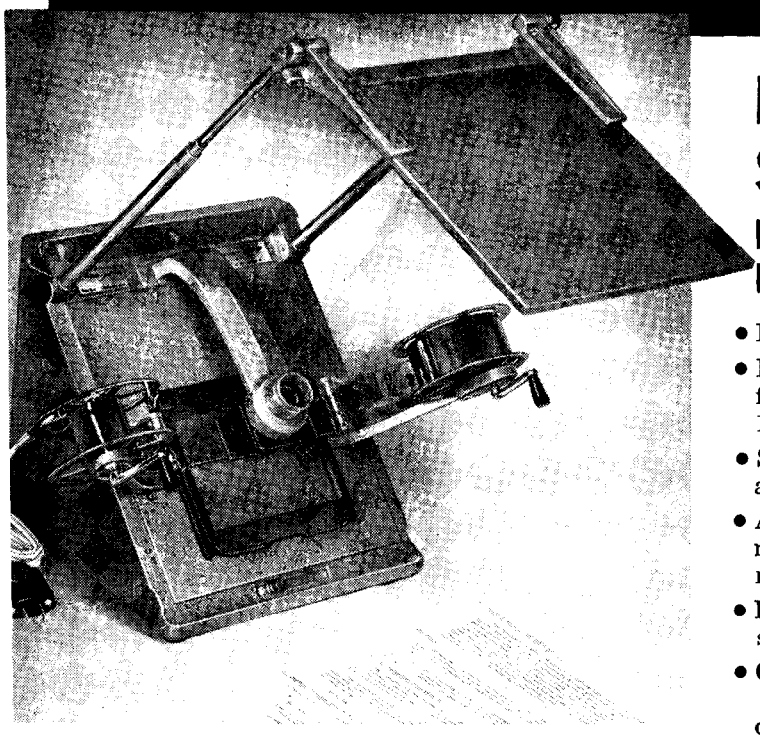
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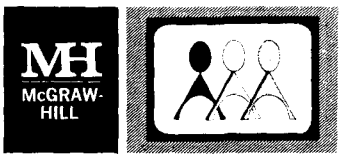
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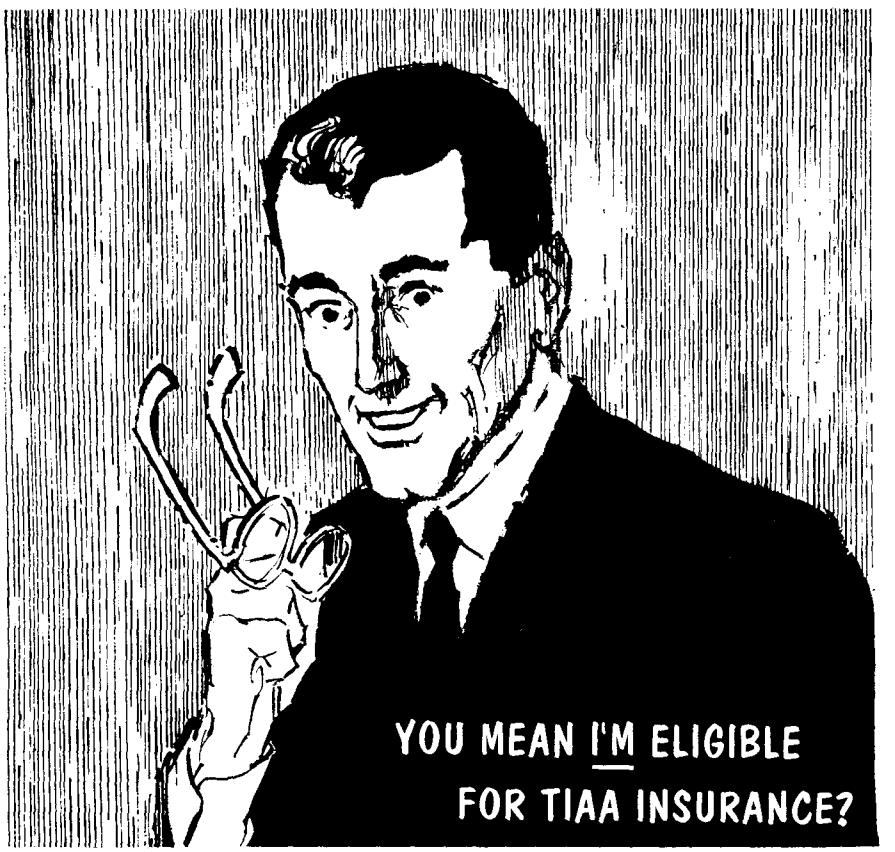
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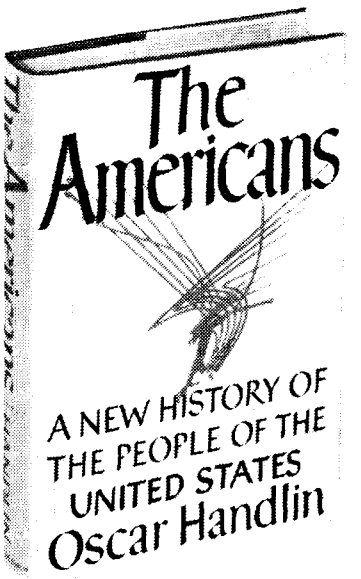
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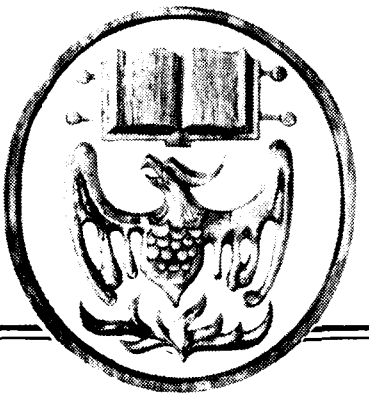
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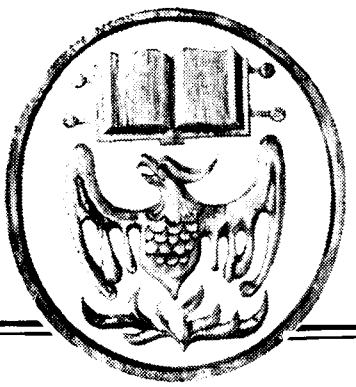
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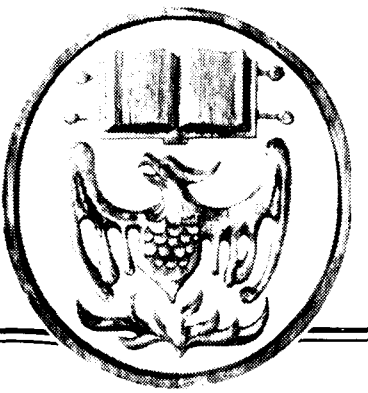
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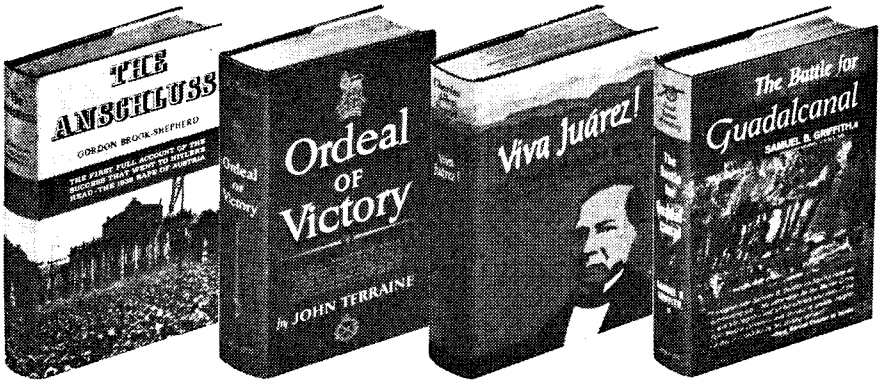
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